

THE  
PHILOSOPHICAL MAGAZINE:

COMPREHENDING  
THE VARIOUS BRANCHES OF SCIENCE,  
THE LIBERAL AND FINE ARTS,  
AGRICULTURE, MANUFACTURES,  
AND  
COMMERCE.

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BY ALEXANDER TILLOCH,

HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

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“Nec araneorum sane textus ideo melior, quia ex se fila gignunt. Nec noster ]  
villior quia ex alienis libamus ut apes.” JUST. LIPS. *Monit. Polit.* lib. i. cap. i.

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VOL. XV.

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# THE HISTORY OF THE

REIGN OF

CHARLES THE FIRST

BY

JOHN BURNET

OF LINCOLN'S INN

ESQ.

IN TWO VOLUMES.

LONDON

Printed by J. Streater

at the Sign of the Gun

in St. Dunstons Church

near St. Dunstons

in Fleet Street

1643.

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THE  
PHILOSOPHICAL MAGAZINE.

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I. *On Indian Dogs.* By Dr. BARTON, of Philadelphia\*.

IT would require much observation, many experiments, and a great deal of time; to collect all the necessary materials for an exact history of the native dogs of North America. I am by no means prepared for the ample task. My object, in the present article, is much more limited—to bring together a number of scattered facts relative to the origin and manners of the different kinds of dogs which were found among the Indians when the Europeans first took possession of the countries of North America. In the investigation of this inquiry I cannot pretend to be very methodical: nor do I expect to avoid errors. I aim, however, at correcting some of the errors of preceding writers.

It has been asserted, by many historians and naturalists, that there were no dogs in America prior to the discovery of this portion of the world by the Europeans†. Mr. Pennant, one of these naturalists, remarks: “As it is certain that the dog of North America, or rather its substitute, on its first discovery by the English, was derived from the wolf tamed and domesticated, so it is reasonable to imagine that of South America had the same origin‡.” I do not think it sufficiently ascertained that there were not originally in America any species of dog of the same stock as those of the old world. It has not yet been proved, that the dog of Newfoundland, of which I am afterwards to make more particular mention, was unknown in that island before its discovery by the Europeans. But admitting that the Newfoundland species or variety originated from an admixture of the European dog with the wolf, or some other native animal of the country, it will still appear somewhat probable, that the Indian dogs in many other parts of America were not *specifically* different

\* Communicated by the Author.

† Acosta, Gomara, Herrera, Joannes Fabri, Buffon, Pennant, &c. &c.

‡ History of Quadrupeds, vol. i. p. 237.

from those of the old world. This question is well worthy of the attention of the naturalist, and is even entitled to the notice of the civil historian of the new world.

*Techichi, or Alco.*

In Mexico and in South America there was a small species of dog, which the Mexicans called *techichi*, and the Peruvians *alco*, or *alco*. This is particularly mentioned by the jesuit Joseph Acosta \*, and other of the earlier visitors of America. The *alco* had a melancholy aspect, and was perfectly mute, or dumb. Hernandez speaks of it as being similar in nature and manners to the common dogs of Europe, and not very different in form †. It is remarkable that Linnæus has taken no notice of this species, though he so frequently refers to the work of Hernandez. Mr. de Buffon has confounded it with the *itzcuintepotzotli*, or next species, from which, however, it appears to have been distinct ‡. Gmelin, who has fallen into a similar mistake (though the words of Hernandez are sufficiently plain), considers the *alco* as a variety of the *canis familiaris*, or faithful dog. He calls it *canis familiaris, Americanus*. Although I have little doubt that the *alco* was a true *canis* §, I think it is too slightly mentioned or described to enable us to determine, with as much certainty as the naturalist could wish, whether it was merely a variety of the common dog, or an entirely distinct species. It is to be regretted, indeed, that the naturalists who visited America in the sixteenth century, whilst the *alco* was still a common animal, have left us so much in the dark concerning its origin and nature. Owing to their negligence, we are, at this distance of time, only permitted to say, with some degree of probability, what it was not. I do not, with Mr. Pennant, think it probable that it was derived from the wolf. Its entire muteness is, I think, greatly opposed to this idea;

\* The Naturall and Morall Historie of the East and West Indies, &c. p. 301, 302. English translation. London 1604.

† Historiæ Animalium et Mineralium Novæ Hispaniæ Liber Unicus, &c. p. 6, 7.

‡ Histoire Naturelle, &c. tom. xxx. p. 200, &c.

§ Independent of the testimony of Hernandez, there would seem to be very little doubt that the *alco* was a true species of *canis*, and, indeed, very similar to some of our small house-dogs. The Spaniards, according to Clavigero, gave it the name of *perro*, which signifies a dog; and Acosta observes, that the Indians called all the dogs which were brought from Spain *alco*, from the resemblance between them and their native animal. It is probable that the Indians in some parts of South America had afterwards (perhaps when the *alco* became either very rare, or extinct) adopted the Spanish word *perro* for dog. The Jaioi, in Guiana, used this word at least as early as 1633. De Laet's Novus Orbis, p. 643.

as is also, perhaps, its complete domestication. If it were immediately derived from the wolf, we ought, at least, to allow the Americans some share of merit for the successful pains they had taken to reclaim this animal from the wild ferocity of his nature.

This species or variety of dog appears to have been pretty extensively diffused through the southern parts of the continent of America, and certainly existed in some of the islands when they were first discovered by Columbus. Peter Martyr, and after him other historians, mention the discovery of mute little dogs in the island of Juanna in the year 1492. Martyr says they were deformed in shape, and that the savages ate of them as the Europeans did of goats\*. I cannot find that this species was discovered among any of the Indian tribes inhabiting the tract of country now called the United States. It is not certain, indeed, that the dogs which Soto found in Florida were not of the alco kind: I think it highly probable, however, that they were not, but that they were much more allied to the wolf and fox, like the modern Creek dogs, which I am afterwards to mention.

It would appear from Clavigero, that the alco is now entirely extinct. "After the conquest of Mexico," says this author, "the Spaniards, having neither large cattle nor sheep, provided their markets with this quadruped; by which means the species was soon extinct, although it had been very numerous †."

*Itzcuintepotzotli.*

The *itzcuintepotzotli* was the Mexican name for another species or variety of dog, which is figured and described by Fabri ‡ and by Clavigero, whose figure is borrowed from that of the Italian naturalist. If the figure be an accurate one, the animal must have been of a very deformed aspect; and as such, indeed, it is described. It was about the size of a Maltesan dog, or rather larger. The head was very small, the ears pendulous, and the eyes soft and pleasing. The nose had a considerable prominence in the middle, and its tail was very small. But the most striking feature of the animal was a protuberance upon its back, not unlike that upon the Arabian camel. The skin was varied with white, tawny, and black.

This species particularly abounded in the kingdom of Mi-

\* The Decades of the Newe Worlde or West India, &c. The first decade, p. 15. English translation. London 1555. 4to.

† The History of Mexico, vol. i. p. 40.

‡ Rerum Medicarum Novæ Hispaniæ Thesaurus, &c. p. 466, &c.

chuacan, the most westerly part of the old empire of Anahuac. The natives of Michuacan called it *abora*, or *abora*. It is said, by Clavigero, to be almost wholly extinct\*.

The *itzcuintepotzotli* bears no resemblance whatever to the wolf, from which it is not probable that it was derived. It has much more the aspect of some of the domesticated dogs; and Hernandez informs us, that it resembled them in nature and in manners. That it was a species of *canis* is very probable; but that it was a mere variety of the common dog is much more uncertain. I rather suspect it was not. We are not, indeed, permitted to decide this matter with certainty. One essential difference between the two animals we are able to collect: the Mexican dog is said to have six teats, whereas the common dog has ten.

Buffon, Pennant, and Gmelin, have confounded this animal with the *alco*. Hernandez, however, plainly speaks of them as two distinct animals†; as does also the abbé Clavigero‡. It is probable, however, that they were considerably allied to each other.

Either this species or the *tecbichi*, perhaps both, were brought to the market of the city of Mexico, along with deer, rabbits, and many other animals, before the conquest of the Spaniards. Gage says these animals were sold either “by quarters or whole.” It would appear from the same writer, that these dogs were sometimes castrated for food§. These simple facts are calculated, with many others, to show that the Mexicans, at the time they were discovered, had actually advanced, in many respects, towards the attainment of that police, those arts and practices, which are never observed among people in the savage forms of society. Another century, but for the discovery of Columbus, would have conducted these unfortunate Americans much nearer to the condition of their conquerors; but it is to be feared that many centuries would have been requisite to have weaned them from their hideous religion, which was the foundation of their savage practices and manners. The history of mankind exhibits abundant proofs of this position, that the arts which they practise, and the police which they observe, are no certain evidences of a truly civilized state. A mild religion appears to be absolutely necessary to the attainment and the preservation of this happy state of man.

\* The History of Mexico, vol. i. p. 44.

† Historiæ Animalium, &c. Liber Unicus, p. 7.

‡ The History of Mexico, vol. ii. p. 382 and p. 323.

§ A New Survey, &c. p. 111.

## Wolf Dogs.

We know not whether the *tecbichi* and the *itzcuintepotxotli* were found in any of the countries considerably to the north of Mexico. We are well assured, however, that different kinds of dogs were very common in many of the countries of North America, when this continent was first discovered by the Europeans, in the 16th and 17th centuries. I am even inclined to think that North America was much better supplied with dogs (I mean these animals in the domesticated state) than South America and Mexico. There seems to be little doubt that in the northern countries there was a greater variety than in the southern countries. Florida abounded in these animals. When Fernando de Soto marched his army through that country, in the year 1540, the Indians supplied him with great numbers of dogs. On one occasion, an Indian cacique sent the Spanish general no less than three hundred dogs\*. These were eaten by the Spaniards, who deemed them not inferior to the best of sheep†. But we are informed that the Indians did not eat them‡. It would seem that the Spaniards did not always stand upon the ceremony of waiting to have the dogs presented to them. The Portuguese author of Elvas, who accompanied Soto and his successor in their mad ramble, informs us, that, during the time the army laboured under a scarcity of meat, “he who could catch a dog in any village thought himself a very happy man; for sometimes (he observes) we found thirty in a place; but the soldier that killed one, and sent not a quarter to his captain, suffered for it, paying dear for his incivilities when he was to go sentinel, or upon any guard of fatigue§.”

We are not told what kind of dog it was that the Spaniards

\* This was the cacique of Quaxule, which, if we can depend upon the old maps of Florida, was in the country of the Chikkasah Indians. A Relation of the Invasion and Conquest of Florida, &c. &c. p. 71.

† See A Relation, &c. p. 55.

‡ A Relation, &c. p. 71. I do not think it certain that the Indians did not eat their dogs. The present which Soto received at Ocute, in the country of the Creek Indians, rather favours the opinion that they did. The cacique sent the Spanish general “two thousand Indians, with a present of rabbits, partridges, mæs-bread, two pullets, and a great many dogs.” A Relation, &c. p. 55. If the Indians did not eat their dogs, why did they suppose the Spaniards were fond of them? It is true, there was a great scarcity of meat and salt at Ocute, and the Indians may have supposed that any kind of food would be acceptable to an army of hungry men. Besides, it is probable they had many opportunities of seeing the Spaniards employed in stealing their dogs.

§ A Relation, &c. p. 56.

found among these Indians. There do not appear to be good grounds to suspect that they were of the *alco* or *itzcuintepotzotli* kinds. It is certain that neither of these animals is now known among any of the Floridian Indians; and it does not seem likely that the breeds which these Indians at present possess have been reclaimed from the wild state since the time of Soto's "mad adventures."

The dogs which are now in use among the Creeks, Chik-kahah, and other southern tribes, are of different kinds. As far as I have been able to collect information concerning them, they, in general, bear a very strong family resemblance to the wolf. One kind is very similar to the *canis lycaon*, or black wolf, of which I have already made mention. It is not, however, always black, but of different colours, commonly of a bay colour, and about one-third less than the wild black wolf. It carries its ears almost erect, and has the same wild and sly look that the wolf has\*.

The other kind of dog is smaller than the one just mentioned, and is more like the common red fox. Both kinds bark, but not so much as the common dogs; and their bark is different from that of our dogs, being more nearly allied to the howl of the wolf†.

I am unable to say, with certainty, whether these southern dogs differ very essentially from those among the northern Indians. I rather suppose they do not. I know, at least, that among the latter, as well as among the former, there are two species or varieties; one which has generally been considered as the wolf merely altered by the domesticated state, and the other more allied to the fox. But as my information concerning the northern dog is more correct and particular than it is concerning the southern, I wish to be understood as speaking principally of the former, in the following description of the Indian dogs.

The Indian dog (I mean that which is most allied to the wolf) is frequently called, by the traders and others, *the half-wolf breed*. His general aspect is much more that of the wolf than of the common domesticated dogs. His body, in general, is more slender than that of our dogs. He is remarkably small behind. His ears do not hang like those of

\* From the information of Mr. William Bartram.

† I have been informed, that among the Cherake Indians the dogs are of a more mixed breed, more like those of the whites. This is doubtless owing to the greater intercourse which has subsisted between these Indians and the whites. The Cherakee themselves are so much mixed with the Europeans that they are often named by the traders, the "Breeds."

our dogs, but stand erect, and are large and sharp-pointed. He has a long small snout, and very sharp nose\*. His barking is more like the howling of the wolf. When attacked, and when fighting, he does not shake his antagonist, like our dogs. His teeth are very sharp, and his bite sure. When he snarls, which he is wont to do upon the slightest occasion, he draws the skin from his mouth back, presenting all his teeth to view. Our dogs, when once attacked by these Indian dogs, always fear and shun them. It is a very curious circumstance, that the Indian dog will never attack or pursue the wolf, which the common dogs so readily do. This fact seems to point very strongly to the origin of the American animal. For the purposes of hunting, the Indian dogs are very useful; but, in other respects, they are by no means so docile as the common dogs. They have less fidelity; for, though never so well fed, they will steal from their masters†. In short, every thing shows that the Indian dog is a much more savage or imperfectly reclaimed animal than the common dog.

If my information has been correct, this species or breed is still preserved in the greatest purity among the Six Nations, from whom the Delawares acknowledge that they received it. The Delawares call this dog *lenchum*, or *lenni-chum*; which signifies “the original beast.” The Nanticokes call him *ihn-wállum*; the Mahicans, *annun-neen-dee-a-oo*, or “the original dog,” to distinguish him from our common dogs, which they call simply *dee-a-oo*, or *de-a-oo*.

These appellations show that the Indians consider their wolf dog as a native of the country, and that they are not incapable of discerning the differences between this animal and the greater number of the varieties of dogs which have been introduced into America by the Europeans.

The origin of the Indian dog is a question of much more difficulty than some naturalists have imagined. Thus, Mr. Lawson seems to suppose that the dogs which he saw among the Indians of North Carolina were merely wolves, “made tame with starving and beating‡.” This is easy natural history. Mr. Pennant, as we have already seen, supposed that the dog of North America was derived from the wolf, tamed

\* Some persons inform me, that many of the Indian dogs have a large white spot upon the breast.

† This assertion, I must confess, is opposed by the testimony of some writers. Thus Carver says, the Indian dogs are “remarkable for their fidelity to their masters; but, being ill fed by them, are very troublesome in their huts or tents.” Travels, &c. p. 416.

‡ A New Voyage, &c. p. 18.

and domesticated \*. This opinion has been maintained by other writers; but it is an opinion which must be admitted with some limitation. I am, indeed, much inclined to believe that the Indian dog, in many parts of North America, was derived from the wolf; but it remains to be proved that it is, in any part of the continent, the pure or unimixed wolf, in a state of domestication. It is, more probably, an hybrid animal, begotten between the wolf and some other animal, perhaps the fox. Mr. Josselyn, a long time ago, considered the dog of the New-England Indians as the produce of the wolf and fox †. This is also the opinion of many well-informed persons who have resided among, or visited, the Indians. I believe it is the opinion which many of the Indians themselves entertain concerning the origin of their dog.

I have already observed that the Indian dog is sometimes called *the half-wolf breed*. This plainly shows that those who have imposed this name did not view the American dog as a mere domesticated wolf. In other words, it shows that they considered him as an hybrid animal. I may add, that Carver and other writers, who have enjoyed pretty extensive opportunities of observing the dogs of the Indians, merely speak of their resemblance to the wolf, without pretending to assert that they are only domesticated wolves ‡.

Owing, however, to the great affinity which subsists between the Indian dog and the wolf, the savages in some parts of North America bestowed the same name upon both of these animals. Thus father Hennepin expressly informs us, that *chonga* is a dog or wolf, in the language of the Iffati and Naudowessies. In general, however, the Indians apply different names to the wolf and to the dog, whether it be their own (or native) dog, or those varieties which they have received from the whites. I may add, that the Indians seem also to have remarked the resemblance of some of their dogs to the fox; for the Mohawks (or at least the Cochnewagoes, who have sprung from the Mohawks) call the red fox *cheets-hoo*. Now the Tuscaroras, who speak a dialect of the language of the Mohawks, call a dog *cheeth* and *cheeth*. Much dependence, however, should not be placed upon this application of names; for savages sometimes bestow the same names upon species that are unquestionably distinct.

\* See p. 1.

† “The Indian dog is a creature begotten betwixt a wolf and a fox, which the Indians lighting upon, bring up to hunt the deer with.” Josselyn’s *New England’s Rarities*, &c. p. 13.

‡ “The dogs employed by the Indians in hunting appear to be all of the same species; they carry their ears erect, and greatly resemble a wolf about the head.” These are Carver’s words. See his *Travels*, &c. p. 416.

We are not yet prepared, it is obvious, to give an exact genealogical history of the Indian dog. We are compelled to mix conjecture with fact. The anatomical structure of the animal should be examined. But, whatever may have been the origin of this breed of dogs, I am disposed to think, with Josselyn, that the savages found it in the woods, and that it has existed as a distinct species, or breed, for a very long period of time. Several of the earlier visitors of different parts of North America speak of the existence of wild dogs in the country. Renatus Laudonerius invaded Florida in the year 1564, only a few years after the death of Soto. In his enumeration of the native productions of the country he mentions wild dogs. There is no reason to suppose that he has confounded them with the wolves: for he expressly says that the country produced, beside these dogs, some species of wolves\*.

The discoverers of the island of Cape Breton, in the Gulf of St. Lawrence, found in that island black dogs, which, we are informed, the Indians were very careful to bring up to hunting†. I think it probable that both these and the dogs mentioned by Laudonerius were the same as the half-wolf breed which I have described.

[To be continued.]

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II. *An Account of a new Method of supplying Diving-bells with fresh Air.* By ROBERT HEALY, A. B.

SIR,

To Mr. Tilloch.

Dublin, Jan. 8, 1803,  
43, James's-street.

I TAKE the liberty of communicating an experiment on diving, which was made last August by my father, Mr. Samuel Healy. Should it appear worthy of holding a place in your very useful and instructive Magazine, you are at full liberty to insert it.

ROBERT HEALY.

THE method of supplying a diving-bell with air, which has hitherto been generally adopted, renders it almost impracticable either to descend in water to any considerable depth, or to remain there a length of time sufficient to perform any thing useful. Much inconvenience results from the attention which it is necessary to pay in admitting the supply from the barrels, which are used as reservoirs of air; much also from

\* See De Laet's Novus Orbis, lib. iv. p. 215.

† See the same, lib. ii. p. 37.

the labour and time which are expended in causing them to reach the bell. The mode of supply which Mr. Healy contrived appears much calculated to remedy these inconveniences. A recital of the circumstances that attended Mr. Healy's experiment, it is hoped, will not prove uninteresting.

Captain Lonsdale, of the Experiment, was employed to raise a brig which had foundered, in the year 1799, in the Bay of Dublin, between Dunleary and Howth. This he endeavoured to accomplish by fastening chains round the bow and stern of the sunken vessel, and by connecting to these a ring on each side, through which cables passed, and were lashed at low water across the deck of his ship, that acted as a buoy. Consequently, on the tide rising, if the fastenings had not given way, the floating vessel must either have sunk itself, or drawn the other upwards.

Mr. Healy accompanied captain Lonsdale in this attempt, in order to put in practice his mode of supplying a diving-bell with air. The bell, which resembled a truncated cone, was made of wood, consisting of staves united by cooperage: the mouth was two feet and a half in diameter; the top one foot and a half: the height was four feet. Windows were placed at proper distances round the sides, and one at the top: there was also an aperture in the top for letting out foul air. In the inside was suspended a stage for the purpose of resting on. On the top was fixed an iron eye, through which a cable passed for raising or lowering the bell. This eye was secured to the bell by four iron bars, of an inch square, that went down the sides and lapped under its edges. Within six inches of the bottom was fixed a broad iron hoop, of an inch thickness, from which weights were suspended to sink the bell.

On the ship's deck was lashed a forcing or condensing syringe, capable of containing about two quarts, to which were connected five fathom of iron tube, and to the end of this an equal length of leathern tube that turned into the bell. When the piston was depressed, the contained air passed through the tubes, and was forced into the bell. Thus a constant stream of air was forced down.

Four hundred weight and a half being suspended in the manner described, Mr. Healy was first let down rapidly, when, on signifying his desire of ascending, his wish was immediately complied with. He stated, that great uneasiness was felt, particularly in his ears, from such a sudden descent; that there was sufficient light in the bell to enable him to read by light reflected through the mouth of the bell without removing the shutters of any of the windows; and also that

he

he could see a good way down in the water. Having rested a few minutes, and matters being arranged more satisfactorily, he was lowered gradually, and remained stationary about a minute at each fathom's depth; the syringe supplying such a constant stream of air, that the bell was supercharged, and the signal of sufficiency was often repeated. Having been more than half an hour down, and for some time on the deck of the sunken vessel, he gave the signal for ascending, and was drawn up in the same gradual manner in which he had been let down.

He said, that the very instant the mouth of the bell was immersed a noise struck his ears, which went off upon his resting at the distance of a fathom from the surface: the next descent caused the same deafening sensation, the removal of which was effected, to a considerable degree, by repeated yawnings. No inconvenience was felt by his respiring air condensed by the pressure of six or seven fathoms water. A slight giddiness remained a few minutes after his emerging, and the blood-vessels about his face were a little swollen.

In the night, the lashings having given way at high water, one of the rings through which the cables passed fell to the bottom. On the following day he descended, in a gradual manner as before, to the depth of seven fathoms, in order to raise the ring, and remained below for an hour and some minutes. Not the smallest inconvenience was felt in his breathing. After coming up he coughed, and a slight tinge of blood appeared in his spittle. The fullness in his face, as also the giddiness, occurred as in the last experiment. In this the bell did not appear to admit so much light as in the former experiment, although the papers explanatory of the communication of signals by pulling the ropes (which, for precaution, were fixed to the inside of the bell) were still legible. With the syringe one man supplied seven gallons of air in a minute, and, if necessary, could have supplied double that quantity.

The following morning the wind rose to such a degree as to break both the cables and chains that were attached to the sunken ship, and of course interrupted the prosecution of further experiments; which, however, at a future period, Mr. Healy hopes to resume. It is to be wished that other adventurers will make similar attempts, and improve on the hint which this trial affords; for, to use the words of Seneca,

*Patet omnibus ars, nondum est occupata, multum ex illa etiam futuris relictum est.*

III. *On Painting.* By Mr. E. DAYES, *Painter.*

## ESSAY VIII.

*On Manner.*

Peculiar marks I hold to be generally, if not always, defects; however difficult it may be wholly to escape them. *Sir Joshua Reynolds.*

THE word *manner* may be applied to colour, light and shade, and penciling. It is expressive of certain peculiar marks that invariably characterize the works of each individual, as in some a blueness in the colouring prevails, in others a gray or yellow, while others are distinguished by a harshness in the shadows; in one the penciling is round, in another square or forked. So far is a new manner from being a mark of genius, as some assert, that, could perfection in painting ever be attained, it would be unaccompanied by any peculiarity whatever.

In that part of our education which is to be obtained by copying, we ought to be particularly careful that the works we copy, or the master we imitate, have a manner the purest and the least vicious possible; for we may rest assured, singularity, which in some is glaringly absurd, will be the first portion we shall inherit.

He who forms to himself a model in a master will be always inferior to the archetype: the heads of all the great schools have been superior to their imitators. Nature rises in the same degree over the servile and base. M. Angelo was superior to his disciples, and that in proportion to their dependent habit of thinking. The same cause placed Raphael, Titian, the Caracci, &c. at the head of certain classes of artists, many of whom have followed their masters limpingly and awkwardly. The same baneful desire of imitation is equally detrimental to poets; for the arts cannot be called liberal in the hands of those who want spirit to think for themselves. Not to acknowledge the favours we receive would be illiberal, but to sink under them into a state of slavery is base. The wisdom of the world may inform, but we must improve from ourselves; for precept will do but little if the mind is not susceptible of it; the seed must be suited to the soil. The mind, like a fine spring of water, will become more productive and clear the more it is used.

Peculiarity is what chiefly characterizes the different masters. We say, for instance, the manner of Raphael is dry

and hard, thereby indicating the too violent display of outline, the decisions and abrupt divisions in the shadows, with the parts not sufficiently lost in their grounds. That of Spagnolet is forcible, with much red in the flesh; while in that of Rembrandt we expect little light, with a glow of colour, that may too often be called rotten-ripe. We may further observe the dark manner of Guercino and the silver of Guido, with many others not necessary to mention. What are those differences in the various masters, but so many singularities that characterize and distinguish the individual?

As perfection cannot be attained, every artist, of necessity, will have a manner; but in proportion as he succeeds in approaching perfection will his manner become the more pure. This he can only hope to attain by an extensive inquiry, that is, by not slavishly tying himself down to the imitation of an individual. He is bound to show in his works that he has opinions of his own, and that he dares to think for himself.

Raphael's first manner was like that of his master Perugino; but this he soon quitted for a sweeter mode of colour, which he caught from Da Vinci and Bartolomeo, and for a more noble and elevated style, which he acquired from M. Angelo. Julio Romano imbibed much of the fire of his master, as did Rosso and Prematicio of theirs. The Caracci adopted a most liberal manner of imitation, founded on a combination of the excellences of all the great schools, and ultimately produced one entirely new. Domenichino, Guido, Guercino, and Schidone, exhibit in their works but slight traces of the school they were formed in. Van Dyck is perfectly original. Our countryman Reynolds is an example of this liberal manner of imitation; we see in his works the grace of Corregio and Parmegiano combined with the beauty and richness of Venetian colouring, accompanied with the *chiaro-scuro* of the Flemish and Dutch. Le Sueur's first manner resembled his master Vouet, which he soon quitted, and made himself superior in every part of the art. Le Brun was influenced by the same spirit of independence, and left his master far behind.

Of those who have been destitute of the pride of independence, and have fallen into a narrow, confined, and illiberal kind of imitation, we will point out a few. Guido was imitated by Sirani, Poussin by Verdier, Paul Veronese by his brothers, and Jacomo Bassan by his sons. Rubens was imitated by Jacques Jordans, who has increased the excesses of his master without adding one perfection of his own. Rembrandt was followed servilely by Bramer, Eckhout, De Gelder, &c. We might enumerate many others, which must occur

to every one's recollection, whose works pass with the ignorant for those of their masters. Perhaps this difference in the works of artists may arise from the external objects and their images painted in the eye of some, not agreeing; which may also account for the variation we observe between the bulk or altitude of bodies and their representations by various artists. This peculiar habit of seeing may constitute what we call manner.

It is certain too much copying, or too great a devotion to the works of some favourite master, brings on a habit of seeing even nature with his imperfections. Hence it is, that what is termed the schools have in the end proved the ruin of art; not because they were bad, but because weak men have been content with nature at second-hand: some author calls such people not nature's sons, but her grandsons.

Before we quit this part of our essay we will just touch on the manner of penciling, also called handling. The use of the pencil is distinguished into the smooth or mellow, and the expeditious or bold. The application of those manners must, in a great degree, depend on the size of the picture to be painted, also its situation and subject. As far as subject is concerned, the former method best applies to objects in themselves beautiful; as elegant female figures, young and sleek animals, pastoral, and all objects intended to delight. The latter manner will best associate with aged, broken, and irregular objects, and such scenes as are intended to excite terrible emotions. The bold pencil of Rubens is highly admirable, and is a great beauty in his pictures; while the pencil of Van Dyck is more soft and mellow. Claude had a smooth pencil suiting his choice of subject; on the contrary, Salvator Rosa is bold, and well impasted with colour. The pencil of Titian is a fine example for the heroic in landscape, or what may be termed the historical style, and which, as mentioned in our first essay, derives its name from the dignity of the objects of which the picture is composed. Those are mistaken who imagine it derives its character from lightness and indecision, and who dignify their own crude and undistinguishable masses with that noble appellation. The pencil of Titian is firm, bold, and at the same time decisive, and, in the higher style of landscape, may be offered as an example worthy of our attention. We shall find numberless examples in the Dutch school to direct our hand in the pastoral; and the pencil of young Teniers is, perhaps, the finest in that part of the art. The pencil is not a mere vehicle for laying on the colour, but its motion must express the character of the various objects it may be employed on. As in

landscape the foliage of trees, incrusting of the bark; on cattle, in representing hair and wool; the character and folds of drapery, the thinness of flowers, &c. &c.

As well as the manual practice, painting requires, 1. A boldness of hand in the dead-colouring: 2. In the second colouring, more circumspection and labour: and, 3. Thorough patience and attention in the retouching and finishing of the picture. These qualities can no more be separated than Venus and the Graces.

He who wishes to insure himself a good manner of penciling should avoid copying such pictures as are imperfect in that respect; for, if we begin with a slovenly or bad one, every step will plunge us deeper in error. But all attempts at painting will be vain if we do not possess the power to determine the form at once, which can only be acquired through a previous practice in drawing; for on this the clearness as well as the firmness of the penciling depends.

There are two methods of preparing our picture: one is, to draw in the objects with their ground colour, carefully laying in the shadows, dead-colouring the lights solid, and *preserving the shadows transparent throughout the work*; in the second stage, to correct the forms, and add to the darks where wanted; and, thirdly, after oiling out the parts, bringing the whole into harmony by glazing, scumbling, and finishing with the extreme lights and darks. This direction is general; for, after the lights and darks are added, parts may require to be kept down, enriched, &c., as practice must direct. The second method is to dead-colour *solidly throughout*, and to finish the shadows by glazing. The latter method admits of greater changes being made in the work during its progress than the former.

\* \* \* In our last Essay, for W. Scope, read W. Scrope, esq. of Castle Comb.

#### IV. *Experiments on the Colouring and Mordant Property of the Oxide of Molybdena.* By M. D. JAEGER \*.

THESE experiments were made with foliated molybdena of Altenberg, in Saxony, prepared in the following manner:

After having separated the metal from all quartz parts, it was reduced into an acid by long exposure to a red heat in an open crucible. This acid was then dissolved in boiling

\* From Scherer's *Allgemeines Journal der Chemie*, January 1802.

water, and saturated with potash. The molybdate of potash being soluble in a much larger quantity of cold water than the free molybdic acid, this acid was united to potash with a view that a more concentrated solution of it might be applied to the stuff, which, in regard to linen and cotton, is a matter of great importance.

A. One part of this solution was diluted with ten or twelve parts of rain water; and a shred of raw white woollen stuff, and another of white Saxon kerseymere, were boiled in it for half an hour. The two shreds assumed a grayish green colour. Both of them were then cut into small bits, for the purpose of subjecting them to the following experiments:

These bits of cloth were put, 1st, Into a boiling solution of sulphate of zinc very much diluted: 2d, A similar solution of acetite of lead: 3d, Another of sulphate of copper: 4th, Another of tin in the sulphuric and muriatic acids: 5th, Another of sulphate of iron: 6th, Of tin in simple muriatic acid: 7th, Another of tin in acetous acid: 8th, Another of tin in nitro-muriatic acid by muriate of ammonia: 9th, Into one of cobalt in nitric acid, rendered slightly muriatic by muriate of soda: 10th, And into aqueous tincture of gall-nuts prepared cold, which communicated to woollen stuff boiled in it a russet colour.

The different pieces, after being boiled for a quarter of an hour in solution of molybdena, when washed and dried exhibited different shades.

B. A shred of white cotton cloth was immersed, cold, for twelve hours, in a part of the solution of molybdate of potash much diluted; and it was then removed to a diluted solution of tin in a mixture of the sulphuric and muriatic acids. The stuff, which had not changed its colour in the solution of molybdena, assumed a bright blue colour somewhat dirty.

The above two pieces of cloth which were treated with the solutions of tin exhibited this peculiar phenomenon, that on the edge, when cut, they were of a darker and purer blue than externally. Another circumstance, no less remarkable, is, that these colours experienced from the light an effect contrary to that produced on vegetable colours, which lose their colour by exposure to the sun; while that of molybdena acquires a double degree of intensity. The green shades generally passed to blue; but after some time resumed in the shade, or when exposed to the moist atmosphere, their primitive colour.

However satisfactory these results might be, M. Jaeger carried his researches still further, and endeavoured to obtain colours better defined, and brighter; and particularly pure blue

blue and green solutions of tin were those which seemed most likely to accomplish that end.

C. The solution of molybdate of potash was mixed, in certain proportions, with pure solutions of tin in muriatic acid, diluted in cold rain water. Shreds of raw white woollen stuff, moistened with boiling water, were boiled for half an hour in this mixture. The liquor was decomposed, deposited the tin under the form of a grayish powder, and the residuum assumed an agreeable blue colour, which was communicated also to the stuff, but with a shade less pure.

D. Not satisfied with this result, M. Jaeger tried to prepare the stuff, before it was immersed in the bath of molybdena, with different mordants; such as sulphate and acetite of alumine, diluted sulphuric and muriatic acid, acidulous tartrite of potash, and others: but the result did not answer the object he had in view. He then thought it necessary to employ other means.

E. A solution in excess of molybdate of potash was then mixed with a saturated solution of tin; the mixture was boiled for half an hour in a retort, and was afterwards left at rest for eight or ten days. After this time, the liquid, which at first was of a grayish blue colour, was found changed into a bright dark blue, and had deposited a large quantity of gray oxide of tin. The liquid was decanted, and filtered through double paper.

1st, This tincture communicated a bright blue colour to a shred of muslin, which had been left in it for a quarter of an hour after it had been diluted in two parts of water.

2d, A shred of raw white woollen stuff was so strongly coloured by this tincture, that after ebullition for half an hour it came out dyed of a pure saturated blue colour.

As the tincture did not appear to be exhausted of colour, there were boiled in it for half an hour, each after the other,

3d, A shred of woollen stuff.

4th, A shred of the same kind. The two shreds acquired a blue colour, decreasing in intensity.

To ascertain how far the blue colour of the woollen cloth could be fixed, a shred of stuff was boiled for an hour in one part of blue tincture diluted with half a part of water. Its colour was not darker than that of No. 2.

The dyed shred was then cut into two parts, and one of them was boiled for a quarter of an hour in the same tincture, to which a little of the solution of tin had been added. Its colour this time was more intense.

The colour in these experiments had distributed itself perfectly, and had united in so intimate a manner with the fibres.

of the wool, that the stuff had all the appearance of wool dyed with indigo. After this success, M. Jaeger endeavoured to unite the colour of molybdena with other vegetable colours, and to transport it in that state to stuffs. The results of this labour were as follow :

F. Different shreds of woollen cloth, to which a greater or less blue colour had been communicated by tincture of molybdena, were boiled for half an hour in a cold aqueous infusion of quercitron bark. They acquired different shades of green, but which did not possess all the purity and uniformity which could have been wished. He therefore thought that it would be necessary to change a little his process.

G. Different shreds of woollen stuff were boiled for a longer or shorter time in an infusion of quercitron bark, with a view to communicate to them different shades of yellow. The quantity of blue tincture necessary for each shade of yellow, beginning with the weakest, was added to the infusion, which was in a great part exhausted. In this manner, shades of green, not inferior in beauty to Saxon green, were obtained by an ebullition of half an hour.

H. In other experiments, sometimes the blue tincture and sometimes the molybdate of potash was tried, either by combination with vegetable colours, or alone, as mordants or modifiers. The results in general were very satisfactory. Several of the colours thus obtained were so fixed and unalterable in the sun and air, and by strong acids, that no other colour could be compared with them in this respect.

M. Jaeger terminates his interesting labour by announcing a black colour, furnished to him by a composition of molybdate of potash, acetite of alumine, and logwood. This black having been tried, in regard to durability, as compared with common black, by boiling both for a quarter of an hour in diluted sulphuric acid, that of molybdena was scarcely changed, while the other lost its colour entirely, and passed to a burnt yellow.

The author adds also, that the blue tincture of molybdena, evaporated in a gentle heat, furnished a very fine blue, soluble in water, which might be used for writing, and even for painting, in the same manner as any other colouring juice.

It appears from these experiments that molybdena may become a very valuable colouring matter in the art of dyeing.

V. *Remarks on the present State of Aërostation.* By  
Mr. G. J. WRIGHT.

[Concluded from p. 346 of our last Volume.]

THE machine being constructed, it only remains to fill it with inflammable air (hydrogen gas). The usual method of procuring inflammable air is by the solution of iron in sulphuric acid or common oil of vitriol: for this purpose a number \* of air-tight casks are disposed in circles of ten or twelve in each circle. The casks composing one circle communicate, by their individual tubes, to one centre cask containing water, whose height must be such as that the orifice of each tin tube should be some inches under the fluid †. The centre casks (or *coolers*, as they are technically termed) are the only ones immediately connected with the balloon, and that by varnished filken tubes proceeding from their summits: there must also be a second orifice in the head of each cask composing the circles; each such orifice being for the purpose of introducing the ingredients, and provided also with an air-tight plug. The iron (of which the turnings from the boring of cannon are reckoned the best) being placed in the casks, the diluted acid ‡ (having been previously well mixed in a separate vessel purposely provided) is to be poured upon it, stopping up the orifice with the plug as soon as the smell of the gas is perceived: the quantity of acid first put into each cask must be half the requisite proportion for the weight of iron therein contained; and this rule must be observed throughout the whole number of casks, so that on the second subsequent addition the aggregate quantity of gas shall be produced §.

The

\* This was the method adopted by Mr. Garnerin; but its complexity renders it very exceptionable, as in other experiments two or three casks have been made to answer all that he effects with thirty, and in much less time.

† Without this precaution the inflammable air is liable to carry along with it some of the acid in a vaporous state, which in one instance became gradually condensed in the balloon when arrived to a colder region, deluging the aéronaut with some quarts of this acidulous liquor.

‡ It is necessary to dilute the acid in a separate vessel, not merely on account of the great difference in the respective gravities of the acid and water, which renders stirring necessary, but also on account of the great quantity of heat liberated, which is sufficient to occasion ebullition in some instances, and might derange the apparatus.

§ This is the method adopted by some experimenters; but a more expedient one deserves to supersede it; especially as by nearly the like and undermentioned process Messrs. Baldwin, Sadler, and Lunardi, successfully inflated their machine in two hours.

The iron must be particularly attended to to insure success; whatever is rusty is not only not serviceable, but actually injurious, by generating hydro-carbonate, a gas specifically

The quantity of hydrogen gas developed during the action of diluted sulphuric acid upon iron, varies not only according to the strength of the acid, but the quality of the iron employed. Dr. Priestley (*Experiments and Observations on Air*, vol. vi.) found that annealed cast iron invariably afforded more inflammable air than iron which had not undergone that process; the difference being nearly 1-8th. Thus iron turnings, as being in some degree annealed, gave considerably more air than only cast iron, yet less than iron nails perfectly annealed. Mr. Cavendish also obtained from an ounce of zinc 356 ounce measures of inflammable air; from the same quantity of iron 412 measures; while tin afforded him only half the quantity yielded by iron. Supposing then we are about to inflate a balloon of 30 feet diameter, it will require, agreeably to Mr. Cavendish's experiments, about two ounces and a half of iron for each cubic foot of inflammable air, or 2200 pounds of iron to be dissolved in order to completely fill the machine; and to produce this solution there will altogether be required an equal weight of concentrated oil of vitriol, and six times this weight of water. But completely filling a balloon before its ascension is useless; because the density of the surrounding medium, immediately decreasing, will cause an equally premature expansion of the gas, and require its escape even before it attains 1000 yards height: the proportional product, therefore, of 2000 pounds of metal will be as much as may with propriety be applied to a machine of 30 feet diameter.

Having proceeded according to the rules hereafter laid down for ascertaining the requisite quantity of materials agreeably to their respective purity, provide next two large cylindrical wooden vessels open at the top, and shaped as inverted truncated cones, each vessel capable of containing about 1000 gallons, so that the two may hold in the sequel the whole quantity of materials. In each of these vessels dispose planes of lattice or basket-work, at intervals of six or eight inches above each other (their diameters corresponding to the diameter of the part of the vessel each plane is to rest upon), adding a sufficient number of them till within a foot of the surface at which the diluted acid will stand when the whole quantity shall have been added. The use of these planes is for dividing the iron into many portions, so as to expose as large a surface as possible to the action of the acid: the weight of iron, supposing the vessel to contain ten of these planes, will be 100 pounds upon each; which weight will suffice for keeping them steady, without having recourse to other methods of fixing them to the vessel itself; while the gas will easily find a passage for itself from the lower through the superior planes by the interstices in the basket-work.

Having thus disposed of the iron so as that the two vessels shall contain their specific proportion, amounting together to the whole of the required quantity, provide for each vessel an appropriate head, of somewhat less diameter than its upper rim, that when fixed therein it may rest an inch or two below the level of the same, in order that a thin stratum of water may lie over the whole surface, and render the head completely tight. From the centre of the head must proceed a cylindrical tube of tin six inches in diameter and one foot in height; whence forming a right angle it should proceed about six feet, and then descend by an obtuse angle whose extremity should terminate in an ascending nook, converging till its ex-

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cally heavier than even common air. It must be also totally devoid of grease, as the smallest stratum of greasy matter will effectually secure the iron from being attacked by the acid.

To

treme orifice be diminished to an inch diameter\*, and in such a direction as easily to be placed under, and six or eight inches below, the base of the inverted funnel of the reservoir. Besides this central tube there must be a lateral one of an inch diameter and six inches in height, provided with an air-tight wooden stopper. Through the head must also be fixed a glass or pewter tube bent to form an inverted syphon, the shorter leg of which must be placed so as to be included in the vessel, while the longer leg (of two feet in length) will remain on the outside. A third vessel must also be provided, as a reservoir or cooler, capable of holding from 200 to 300 gallons, open at top, yet not more than three feet deep. Across its diameter, and eight or ten inches below its upper rim, fix two strong wooden bars as supports, to which fix an inverted wooden funnel (the base of which must not be less than two feet diameter) whose upper part should end in a tube, round which the varnished silken tube immediately connected with the balloon is to be fastened. After properly fixing the inverted funnel, provide a square wooden tube, and fix it to the inside of the reservoir so as to reach within six inches of its bottom, while its upper orifice should stand at least a foot higher than the surface of the water therein. This being done, and the extremities of the large tin tubes being properly placed under the inverted funnel; begin with pouring several gallons of cream of lime (made by slaking quicklime in water, a gallon of fluid to two pounds of lime) into the reservoir; then gradually fill the same with water till within an inch or two of the top. In the next place, pour through the inverted syphon of each tub a mixture of one part acid and six of water, while an assistant standing over the small lateral tin tube (now open) replaces the plug in the same as soon as the smell of the hydrogen gas is perceived, indicating that the atmospheric air is now all expelled from the tub. And thus must the workmen proceed, pouring the diluted acid into the tubs through the syphons till such time as the required quantity for each vessel (*i. e.* 1000 pounds of acid, and 6000 pounds, or 750 gallons, of water) has been added; the column of fluid retained in the shorter leg of the syphon forming a stopper, whereby any escape of gas at this aperture is prevented.

A separate assistant must in the mean time attend the pneumatic cooler to renew the water therein, which will be at intervals saturated with all the matter it can absorb from the gas; he must therefore occasionally displace this water by pouring fresh quantities down the square wooden tube for the purpose, which will cause the saturated fluid to flow over the top of the vessel, and in doing the same must, for every fresh addition of water, add a proportionate quantity of the cream of lime. During the whole of the experiment the water in the cooler must be stirred with a wooden rod, that the unsaturated lime (continually tending to precipitate itself) may be perpetually brought into contact with the gas; while, the more frequent the renewal of the water in the reservoir, and the larger the proportion of fresh cream of lime, the lighter will the gas be obtained. Also, to facili-

\* By this contrivance the gas will issue in a continued stream, which will prevent that great agitation of the water under the funnel which is occasioned by the gas forcing its way through the water in large quantities at a time, yet at intervals of some distance, whereby much of it is frequently lost.

To provide against these two circumstances, the aëronaut ought not to omit subjecting the iron turnings to the action of a strong fire amidst carbonaceous matters, for at least an hour, a day or two before his proposed use of them. The thickness of the iron must be also noticed, as, under the most favourable circumstances, the acid never penetrates it more than half an inch: therefore all the weight of iron that remains over and above a surface of half an inch depth every way, becomes useless; the coat of newly formed oxide of iron effectually guarding the remaining metal from solution: for want of attending to this circumstance, the quantity of metal has frequently proved deficient to the end proposed, to the great disappointment of the experimenter.

Also where the public have to rely on the good faith of the exhibiter, he ought to try the strength of his acid previous to employing it; and this, not by the ordinary methods of specific gravity, nor the quantity of alkali it will saturate\*, but by the actual process, in the small way, of what he proposes to perform on the day of ascent: by these means he cannot fail of succeeding.

But the above is not the only method of procuring inflammable air†: the passing of water over red-hot metals is fol-

tate the entrance of the inflammable air into the balloon, the varnished silken tube, proceeding from the inverted funnel, must rather ascend than otherwise in its course to the balloon, which may be easily managed by supporting it along a ladder having a gentle inclination upwards. I apprehend the cost of the larger tubs of 1000 gallons each will be less expensive than to have recourse to twenty or thirty butts for the purpose; which, by dividing the process, causes greater apprehensions for failure with respect to the tightness of so many vessels and tubes; besides the inconvenience of having to procure so great a number of casks for every fresh voyage, when undertaken as a public exhibition in places far distant from each other.

The sulphate of iron, the product of the mutual action of the acid and iron, is a substance much used in the arts under the name of green copperas: the chemist, dyer, calico-printer, ink-maker, refiner, &c. have great demand for this article. It may therefore be either disposed of, or distilled *per se* to regain the acid; while the residuum, by a further calcination, may be converted into that species of oxide known by the name of colcothar, an article much employed for polishing glass and metals, and also as a pigment.

\* We are liable to mistakes if we rely on the specific gravity of the acid (usually varying from 1.6 to 1.8), which is augmented by its containing in solution neutral salts, especially sulphate of potash; an adulteration not unfrequently practised. The quantity of alkali required to saturate a given quantity of an acid, can never detect the absolute quantity of the individual acid we are in search of, but only indicate the degree of acidity of the whole, without any regard to the specific radicals.

† Inflammable air is procured in many chemical operations; but it is useless to mention here any other than such as are advantageously applicable to aërostatic purposes.

lowed by the extrication of a large quantity of hydrogen gas from the decomposition of the water; but the metal is apt to run to a slag resembling finery cinder, preventing its further action upon the water, and that before any considerable proportion is decomposed.

The distillation of pit-coal also affords this gas, but not above one-fourth lighter than atmospheric air. In obtaining it in this way, a number of receivers must be interposed between the retort and the balloon, to condense the volatile, aqueous, and oleaginous products of this distillation\*.

If a parachute is required, it should be constructed so as when distended to form but a small segment of a sphere, and not a complete hemisphere; as the weight of this machine is otherwise considerably increased, without gaining much in the opposing surface. The parachute of Mr. Garnerin is particularly defective in a too great extension of its diameter; an unnecessary addition to its weight of a lining of paper both within and without; the too near approximation of the basket to the body of the parachute; and especially in the want of a perpendicular cord passing from the car to the centre of the concave of the umbrella, by the absence of which the velocity of the descent is certain to be very rapid before the machine becomes at all distended; whereas, if a cord were thus disposed, the centre of the parachute would be the portion first drawn downwards by the appended weight, and the machine would be almost immediately at its full extension.

Having found, by experiment, the diameter required for insuring safety†, the further the basket or car is from the umbrella, the less fear shall we have of an inversion of the whole from violent oscillations; yet the longer the space between the car and the head of the machine, the longer will be the space run through in each vibration when once begun, yet by so much the more will they be steadier; and this ought to be attended to, as when by the violence of the oscil-

\* During the distillation of pit-coal, water and carbonate of ammonia are afforded, together with a pitchy oil resembling tar; these will be condensed in the nearest receivers, while the gaseous product (consisting of a mixture of hydrogen with azotic and carbonic acid gases) will proceed onward; and which if made to pass through lime-water will be rendered more pure: a pound of coal will afford about three cubic feet of inflammable air, and prove the cheapest method of inflation. Dr. Priestley also found that the product of inflammable air, in all operations in the dry way, was always greater in proportion as the fire had been suddenly raised.

† Mr. Baldwin advises the diameter 15 feet; by which means, "the man weighing 140 pounds and the parachute 10 pounds, with a surface of 150 square feet, he would feel no greater shock than if he had fallen from the height of six feet." At the Pantheon, Mr. Garnerin's parachute was announced as being 40 feet in diameter, and weighing above 80 pounds.

lations the car became (in Garnerin's experiment) on a line with the horizontal axis of the machine, (or, in other words, the point of suspension,) the force of gravity, or the gravitating power of the weight in the car, on the umbrella, being at that crisis reduced to nothing, the slightest cause might have carried the body of the machine in a lateral direction, reversing the concavity of the umbrella, and Mr. Garnerin, perhaps, have fallen upon the now convex yet internal portion of the bag, and the whole have descended, confusedly together \*.

I should

\* The umbrella being acted upon in a perpendicular direction from the car, if by any, the most trivial cause, the latter vibrates from the perpendicular it will draw the machine in the same inclined direction; the car also with its cords of suspension, now acting the part of a pendulum, will continue to oscillate to each side till a perfect equilibrium is restored. The space of the arc described in each vibration will depend upon the distance between the car and the parachute conjunctly with the resistance afforded by the latter to the air, whereby on decreasing the resistance (or, which is the same thing, the surface of the umbrella,) the weight in the car will sooner draw the machine into the perpendicular, and the oscillations consequently cease: thus a parachute whose surface is small enough to obviate oscillations in the middle regions (*i. e.* the half of the usual heights surmounted in aërostatic experiments, or about 5000 feet), would be defective for want of surface to moderate the velocity of descent when near the ground, and *vice versa*: if, therefore, we can augment or diminish at pleasure the surface and consequent resistance of the parachute during its descent, we shall have attained the perfection of the machine.

Thus in Mr. Garnerin's experiment the oscillations did not commence till the machine felt the increased resistance of a more dense zone of the atmosphere; and having then no means of diminishing the same, the vibrations began, continued, and increased, till, by arriving in a yet *more* dense region, a greater resistance was opposed to the oscillations of the basket, gradually diminishing the arc of the same to nothing, this also being assisted by a discharge of ballast from the car, by which means the weight of the pendulum and its power to sweep through a dense atmosphere were conjointly diminished; for *the vibration of a pendulum will be in proportion to its length, and also to the density of the medium through which it is to vibrate*; a fact too much overlooked. There is therefore a medium height at which the oscillations will be the greatest; and the distance of this medium from the earth's surface will vary with the different states of the barometer at the earth; or, in other words, as the density at different heights. Thus, when the altitude of an aëronaut descending by a parachute is great, the atmosphere is there too much rarefied to occasion resistance to the umbrella sufficient to allow of an oscillation; but when in a lower zone a vibration has unfortunately begun, the atmosphere is here too dense to allow of that velocity of descent which only could have prevented the commencement of the oscillations, yet not dense enough to afford sufficient resistance to retard the sweep of the basket; while in a yet more dense region, nearer to the surface of the earth, the air affords so much resistance to the swing of the car, as soon effectually to stop the same, especially if the weight therein could now be diminished by a discharge of ballast; a fact evinced in Mr. Garnerin's experiment, when he threw out a quantity of ballast when near the earth to moderate the shock of his descent, which immediately

I should propose the parachute to be constructed of varnished cambric muslin (or light linen), covered externally with a netting, the meshes of which should converge smaller as they approach the central portion of the machine; observing also to fix the netting on the surface by sewing it down upon various parts of the same. The diameter of the parachute (forming only a small segment of a sphere), when extended, not to exceed twenty feet. The central aperture of the umbrella (to allow the escape of the air through, as the machine descends), not less than three feet in diameter, and provided with a shutter moving upon a hinge and opening inwards \*; furnished also with a cord sliding over a single pulley to shut the aperture, and a second cord to open the same (each cord reaching to the car), thus subjecting the shutter to the will of the aëronaut. By this mechanism the stupefying vibrations of the car may be moderated, if not wholly prevented, by drawing down the shutter so as to fully expose the aperture, whereby the resistance of the parachute to the atmosphere being lessened by all this diminution of its opposing surface, the gravitating power of the appended weight (tending to draw the body of the machine continually in the perpendicular) will have proportionally a greater effect: the descent in the interim will be also relatively more rapid; but this may be checked in a moment by closing the orifice, especially when near the surface of the earth †.

A strong cord should proceed perpendicularly from the centre of the car to a point formed by the convergence of a number of cords proceeding from the second ring of wicker-work, the effect of which would be an immediate extension of the parachute to its full diameter at the instant of descent, as before adverted to. Also, and lastly, a third hoop of wicker-work, of six or eight feet diameter, fixed in the internal concave of the umbrella, to secure against any accidental collapse of the sides before the machine has become

diately restored the machine to its perpendicular situation, but for which he could not account. An attempt to obviate these inconveniences is the object of the valve of the parachute mentioned hereafter.

\* This aperture, or, more properly, valve, of the parachute, may be constructed either of close wicker-work, or varnished linen strengthened by a piece of netting externally, and fixed to a ring of wicker-work adapted to close the diameter of the second ring of wicker-work, to which are fixed the pieces of varnished linen composing the concave umbrella. This latter circle is independent of the third hoop of wicker-work of six or eight feet diameter, afterwards mentioned, whose use is to prevent a collapse of the sides of the machine.

† By an enlargement of the diameter of the valve, this diminution of surface might be carried to such extent as to insure a safe descent by the parachute even in the most tempestuous weather.

distended

distended by its pressure on the cylinder of air immediately beneath it.

With respect to the probability of directing aërostatic machines\*, we may infer it to be possible, although the methods hitherto tried have been inadequate; perhaps because they were not sufficiently powerful; as, to expect to make so large a body as a balloon to vary from the wind by the impulsion of an oar of six or eight feet in length and one or two in breadth (and that by only endeavouring to draw the car out of the perpendicular), is to expect, by means of a boat's oar, to impel a ship of burthen. Oars are doubtless the most likely means to effect this purpose, if they were of dimensions proportionate to the effects they are wished to produce†. The addition of sails, where any variation from the wind is desired, will prove injurious till we have attained a method (perhaps only to be accomplished by oars) of keeping the same point of the balloon continually in a given direction. Yet I doubt not but these also might prove of great service in quick dispatches by water; as, for instance, where it is required to pass a fortrefs or fleet for the succour of a besieged town, or convey dispatches thereto: a small balloon, of ten or twelve feet diameter, provided with sails to expose a large sur-

\* 'Tis a matter of surprise, that the various hints for directing balloons appear to lie dormant with their projectors, who seem indisposed to make any attempts to carry their plans into execution: thus the inventions of professor Danzel (*Philosophical Magazine*, vol. iv.), also of Martin, and the proposals for performing the same by means of eagles trained for the purpose; or by a reversed parachute to retard the direct progress of the balloon, whereby less power will be necessary to impel it in a lateral direction; all these plans remain obsolete and unpractised from the time of their suggestion.

† According to the present mode of rowing balloons from the car, whatever deviation is made may be compared to the lee-way of a ship at sea; the power being applied so as merely to prevent at a certain instant the car from following the balloon, which will therefore be impelled, during that short interval, in a barely diagonal direction amounting perhaps, in the end of the voyage, to scarcely half a point from the wind. For oars to produce their full effect they should be not less than twenty feet long, and three in width at their extremities; while the rowers should be seated one on each side of the machine in appropriate cars attached to the body of the balloon, and nearly on a line with its equator: this might easily be practised with a balloon capable of carrying four persons and ballast; the greater weight in the car below the machine, containing two travellers with ballast, being sufficient to keep the whole apparatus in a steady position, and that more especially if the same be suspended at a considerable distance beneath the balloon. The oars recommended (being made of only varnished linen stretched over netting fixed to arms of pliable wood) might be easily worked, if a projecting staff of wood were attached to each lateral oar, so as to form at its extremity a fulcrum whereon to rest the oar at eight or ten feet distance from the rower.

face to the wind, being attached by a long rope to a boat, would outstrip the quickest vessel, and might also be made to deviate from the course of the wind; as the water would form a counter-resisting medium, the want of which in air-balloons occasions the difficulty of steering them. A sail-balloon similar to the above might also be advantageously attached to a land-carriage; namely, by increasing the capacity of the balloon so that its power of ascension being nearly equal to the weight of the appended carriage, the latter would be drawn along by the impulsion of the wind against the balloon and sails, while the friction over the ground, by the small overplus weight, may be reasonably expected to afford a resistance sufficient to guide the machine, and allow of a deviation in the carriage of at least eight points from the course of the wind\*.

To whatever degree of perfection aërial navigation may attain, the limits to which a traveller might soar will for ever be confined to but a small distance, even supposing that man could exist in any station of the aërial regions however elevated. The density of the atmosphere decreasing in a geometrical proportion, it will be found that, if a sphere of sheet copper †, of half a pound to the square foot, were constructed of equal diameter with our earth (= 7920 miles), and totally exhausted of its inclosed air, such a globe would attain its equilibrium at 70'047346 miles distance from the surface of the earth; nor would it attain a greater elevation, although its power of ascension at the instant of departure would be equal to 2871691637967270771712 pounds.

The application of aërostatic machines to the advancement of our knowledge of the various phænomena in meteorology, stands prominent, as the, perhaps, only means of maturing our acquaintance with causes yet known only by their effects. Their use will also be indicated in many urgent cases where other means of conveyance might fall short. At the same time I conclude with remarking, that the hitherto unsuccessful

\* A land carriage to be moved by the wind is not a new contrivance: a machine of this kind, rigged in all respects like a sloop, used occasionally to be experimented upon on Barham Downs, near Canterbury: the carriage being of a light construction, and containing two persons, was found capable of running a mile in three minutes and a half, upon level ground and with a fair wind. Whether it could surmount the occasional irregularities of a common road, or deviate in any measure from the wind, I do not now recollect.

† If metallic aërostats were ever to be constructed agreeably to the scheme of the jesuit Francis Lana, means might be found to exhaust them: but to prevent their being crushed together by the pressure of the atmosphere would be more difficult to accomplish!

ful attempts to render aërial navigation of service to mankind, ought to be no argument for causing it to be discountenanced by men of sense, or prohibited by civil authority; as experiments in any art, however multiplied, if not well advised and conducted, cannot be expected to fulfil the erroneous expectations of their projectors.

To assist the artist in constructing his aërostatic apparatus, it may not be amiss to subjoin the following data:—The diameter is to the circumference of a sphere as 1 to  $3\frac{1}{2}$ , or as 1 to 3.1416. The circumference multiplied by the diameter gives the surface of the sphere in square dimensions, and 1-6th of the surface multiplied by the diameter, or the cube of the diameter multiplied by 0.5236, gives the capacity or solid contents in cubic measure. By the weight of a square foot of the envelope may be found the weight of the whole bag, allowing for the seams; and to find the power of ascension, subtract the weight of the bag and inclosed air from the weight of an equal bulk of atmospheric air\*, and the quotient will give the power which the balloon will exert to rise, which (during its ascent) will decrease till the equilibrium between the two is restored; and this point may be nearly ascertained by recollecting that the density of the atmosphere decreases in the geometrical ratio, and is reduced to half an atmosphere at three miles and a half from the earth's surface†. If a balloon with its annexed apparatus weighs therefore, at the instant of ascent, but half an equal bulk of common air, it will rise to three miles and a half perpendicular height; but, as the density of the inflammable air decreases also in the ratio of the diminishing pressure of the surround-

\* A cubic foot of water weighs 1000 ounces avoirdupoise: and as the density of air to water is as 1 to 800, this will be  $1\frac{1}{5}$  ounce for every cubic foot of atmospheric air: the weight of inflammable air varies according to its purity, so as to be even 17 times lighter than common air; but in the gross way, for aërostatic purposes we must not expect to obtain it more than eight or ten times lighter than atmospheric air.

† Agreeably to the annexed table:

At miles

$3\frac{1}{2}$	the air is	2	times rarer.
7		4	
14		16	
21		64	
28		256	
35		1024	
42		4096	
49		16384	
56		65536	
63		262144	
70		1048576	

ing

ing air, so if the balloon were originally but a quarter full (by which means room would be left for this subsequent expansion of the gas); the machine would mount proportionally higher than the three miles and a half, by as much more as the relative density of the gas is become less. Thus, if, as is now allowed, we reckon the volume of all elastic fluids to be inversely as the pressure, and that the balloon was at first but a quarter full (yet of half its weight of power of ascension), it would not stop till arrived at seven miles height, and the machine would be at its full distension, the density being here lessened four times what it was at the moment of ascension. But if the machine (whose power of ascension is equal to half its whole weight) was at first half full, the gas would be expanded to twice its volume at three miles and a half; the balloon would then be full, and all further rise would be prevented by the air escaping at the neck by all the subsequent rarefaction; and if this was prevented by tying the neck, the machine would burst. But we are prevented from knowing to what precise height a balloon will rise, from the yet unaccounted-for circumstance of the temperature of the contained air always much exceeding that of the surrounding medium: and as there is not that coincidence in the expansions of different gases by temperature, as is the case with respect to barometrical pressure; so, till we are better acquainted with the cause of this diversity of temperature, and the precise expansion of the gas by the varying degrees of the same, we must rest satisfied with a rough calculation in supposing the temperature of the internal and external air to correspond\*.

The annexed table (extracted from Mr. Cavallo's treatise) shows the proportion between the surfaces and capacities of spheres of various diameters, remembering that the specific

\* From the mode of inflating aërostatic machines, and the dissolving power of all elastic fluids, the contained gas will always hold in solution more or less water in an extremely divided state. In the elevated and cold regions of the atmosphere this vaporous fluid will be condensed in small drops in the inner concavity of the balloon; but the before-combined yet now free caloric (to which the fluid owed its vaporous state, and which, in every condensation of a fluid, or change of state of its aggregation from aëriform elasticity to a fluid or solid state, is given out,) will be prevented from escaping to the external air by the non-conducting quality of the envelope: it will therefore serve to augment the temperature of the hydrogen gas; while in a warmer zone (or when a large and white cloud is so situated as to become a mirror to the balloon, strongly reflecting the rays of the sun upon the same,) the contrary effects will ensue—the water will reassume the vaporous state, and the temperature of the gas be proportionally reduced, as far as relates to its contained water.

quantity

quantity of the number in the first column, whether reckoned as inches, feet, or yards, must be the same in each lateral column; as square inches, feet, or yards, in surface—and cubical inches, feet, or yards, in capacity \*.

Diam.	Surface.	Capacity.	Diam.	Surface.	Capacity.
1	3.141	0.523	26	2124	9203
1½	7.068	1.767	27	2290	10306
2	12.567	4.188	28	2463	11494
2½	19.635	8.181	29	2642	12770
3	28.274	14.137	30	2827	14137
4	50.265	33.51	31	3019	15598
5	78.54	65.45	32	3217	17157
6	113.097	113.097	33	3421	18817
7	153.938	179.594	34	3632	20580
8	201.062	268.083	35	3848	22449
9	254.469	381.704	36	4072	24429
10	314.159	523.6	37	4301	26522
11	380.1	696.9	38	4536	28731
12	452.5	904.8	39	4778	31060
13	530.9	1150.3	40	5026	33510
14	615.8	1436.7	45	6362	47713
15	706.9	1767.1	50	7854	65450
16	804.2	2145	55	9503	87114
17	907.9	2572	60	11310	113098
18	1017.9	3054	65	13273	143794
19	1134.1	3591	70	15394	179595
20	1256.6	4189	75	17671	220804
21	1385.4	4849	80	20106	268083
22	1520.5	5575	85	22698	321556
23	1661.9	6371	90	25447	381704
24	1809.6	7238	95	28353	448922
25	1963.5	8181	100	31416	523599

\* I have omitted to particularize the method of cutting the pieces composing a balloon, which will vary according to the width of the stuff; as the smallest ingenuity will enable any one to describe a pattern, only recollecting the length in circumference of the machine required, together with its proposed shape. Also the heights of the barometer are omitted (as requiring corrections for temperature too nice for any but philosophical amateurs, who may, with propriety, be supposed to be already masters of this essential branch of the science) for the present purpose—only recollecting that, without regard to the difference of temperature, an elevation of 16 fathoms, or 96 feet, is usually allowed for the fall of every tenth of an inch of the mercury.

POSTSCRIPT.

## POSTSCRIPT.

So long as the vibration of pendulums is performed in a medium of varying density, we must not look for an accurate time-piece for ascertaining the longitude, &c.; unless a self-correcting mercurial pendulum could be contrived, adapted to counteract the smallest variations effected by the ambient air. The errors of a time-piece are but half corrected by the fabrication of pendulums adapted to obviate the expansion of metals by increase of temperature, if the works themselves still remain constructed of such expansible materials. A correct time-piece, therefore, will be that of which not only the works and pendulum are constructed of the least expansible materials, but the pendulum itself shall vibrate in a medium of unalterable density; a desideratum only to be obtained by causing the vibrations to be performed *in vacuo*, or by a self-correcting pendulum, as above alluded to. I know of no substance so well adapted to compose the works of such a machine as ivory, or the horn of the narwhal or sea unicorn (nearly entirely composed of enamel, and used in the fabrication of artificial teeth), especially if we had a means of increasing their hardness so as to vie with the metals (a subject on which I propose to make some experiments): but for the pendulum itself, the common gridiron pendulum is allowed to be the most accurate in use, provided its vibrations were not obstructed by unequal resistance from the air: but for a mercurial pendulum to move in open space, glass will be indicated as the least expansible and most proper substance for this purpose. I add, that the most perfect time-piece hitherto constructed, has been so only in as far as the works have been most exactly accurate, and the expansion of the pendulum been counteracted (as in the gridiron pendulum) by opposing expansion to expansion. The exact concordance of such a machine, on its return to the first place of observation, will have depended on the nature of the voyage performed in the interim. As, if such voyage has been performed across the equator and back (as to the East Indies), the aggregate number of anomalies in the pendulum in the voyage out, will in the return be nearly balanced by the number of anomalies in the voyage home, so that the whole difference shall at last, perhaps, not amount to 100". But if (some weeks after the sailing of the vessel) we had it in our power to know at a certain instant the precise moment indicated by an equally accurate chronometer left at the place of departure, we should find a greater variation between the two, owing to the difference in the densities of the media in which the pendulum at home

home and that on board the ship have in the interim vibrated. I say then,

1. That the concordance (although exact) of two chronometers, after a voyage on the part of one of them, is no proof of their accuracy.

2. That this concordance will depend on the nature of the voyage that has been performed in the interim. And,

3. That we must not expect an exact instrument till the expansions by heat, and the varying density of the atmosphere, are counteracted in the same machine.

For although the difference of an inch barometrical pressure may not cause an obstruction in the arc of vibration equal to the two millionth part of the whole, yet this small difference (which, perhaps, may have prevented its hitherto due observance,) will, in the space of some months, amount to something considerable, especially as a variation of not only one, but two or even three inches increase or diminution of pressure will frequently ensue in less than 24 hours.

Kennington,  
January 17, 1803.

VI. *A Sketch of the Geography of Cochin China; some Particulars relative to the Manners, Customs, and History of the Inhabitants; and a few Considerations on the Importance of forming an Establishment in that Country\*.*

**COCHIN CHINA**, called by the natives Anam, extends from about the 20th degree of north latitude to Pulo Condore, which lies in  $8^{\circ} 40'$ . It is bounded by the kingdom of Tonquin on the north, from which it is separated by the river Sungen; by the kingdom of Laos, and by a range of mountains, which divides it from Cambodia, on the west; and by that part of the Eastern Ocean generally called the China Sea, on the south and east.

The kingdom is divided into twelve provinces, all lying upon the sea-coast, and succeeding each other from north to south in the following order:

*Ding-oie*, *Cong-bing*, *Ding-cat*, *Hue* (or the Court), in the possession of the Tonquinese. *Cham*, *Cong-nai*, *Quinion*, in the possession of Ignaack. *Pbu-yen*, *Bing-khang*, *Nab-tong*, *Bing-thoam* (or *Champa*), dubious whether subdued by Ignaack, or still in the possession of the king. *Donai*, in the possession of the king.

\* From the *Asiatic Annual Register* for 1801.

The breadth of the country bears no proportion to its length. Few of the provinces extend further than a degree from east to west, some less than twenty miles: Donai, which is properly a province of Cambodia, is much larger.

The whole country is intersected by rivers, which, although not large enough to admit of vessels of great burthen, yet are exceedingly well calculated for promoting inland commerce.

The climate is healthy, the violent heat of the summer months being tempered by regular breezes from the sea. September, October, and November, are the season of the rains; the low lands are then suddenly overflowed by immense torrents of water which fall from the mountains. The inundations happen generally once a fortnight, and last for three or four days. In December, January, and February, there are frequently rains brought by cold northerly winds, which distinguish this country with a winter different from any other in the east. The inundations have the same effect here as the overflowings of the Nile in Egypt, and render the country one of the most fruitful in the world. In many parts the land produces three crops of grain in the year. All the fruits of India are found here in the greatest perfection, with many of those of China.

No country in the east produces richer or a greater variety of articles proper for carrying on an advantageous commerce: cinnamon, pepper, cardemoms, silk, cotton, sugar, Agula wood, Japan wood, ivory, &c. Gold is taken almost pure from the mines; and, before the troubles, great quantities were brought from the hills in dust, and bartered by the rude inhabitants of them for rice, cloths, and iron. It was from them also the Agula and Calambae woods were procured, with quantities of wax, honey, and ivory.

The animals of Cochin China are bullocks, goats, swine, buffaloes, elephants, camels, and horses. In the woods are found the wild boar, tiger, and rhinoceros, with plenty of deer; the poultry is excellent, and the fish caught on the coast abundant and delicious. The flesh of the elephant is accounted a great dainty by the Cochin Chinese. The breeding of bullocks is little attended to; their flesh is not esteemed as food, and they are made no use of in tilling the land, which is performed by buffaloes. They are totally unacquainted with the art of milking their cattle.

The aborigines of Cochin China are called Moys, and are the people which inhabit the chain of mountains which separate it from Cambodia. To these strong-holds they were

driven when the present possessors invaded the country. They are a savage race of people, very black, and resemble in their features the Caffrees.

It was about the year 1280 of the Christian æra that the first Tartar prince became possessed of the throne of China. This revolution afforded an opportunity to the western provinces to throw off their dependence; and they were formed into a kingdom, under a prince whose descendant now reigns in Tonquin, and is called Knaw-Whang. About the beginning of the fifteenth century a large body of people from these provinces, being disaffected to the government, joined under a leader of abilities; they soon became masters of the coast of Cochin China as far as Cape Aurilla, which lies in latitude  $12^{\circ} 30'$  north. The Moys, the original inhabitants, retired to the hills bordering their country to the westward; where they have ever since remained. The emigrants, under their conductor, founded the kingdom of Cochin China. The continual wars they were engaged in with the Tonquineſe, induced them to build a wall on the southern extremity of the province of Ding-noi, to prevent their irruptions. Every communication by sea was strictly forbidden. In the year 1764 the country of Cochin China was in a flourishing condition, and governed by a prince of abilities: soon after his son succeeded to the throne, and anarchy and confusion ensued.

The Cochin Chinese bear evident marks of their being derived from the same stock as the Chinese. They resemble them in their features, and most of their manners and customs. Their religion is the same: their oral language, though different, seems formed upon the same principles; and they use the same characters in writing. They are a courteous, affable, inoffensive race, rather inclined to indolence. The ladies are by far the most active sex; they usually do all the business, while their lazy lords sit upon their haunches, smoking, chewing beetle, or sipping tea. Contrary to the custom of China, the ladies are not shut up; and, if unmarried, a temporary connection with strangers who arrive in the country is deemed no dishonour. Merchants often employ them as their factors and brokers, and, 'tis said, the firmest reliance may be placed on their fidelity.

The habit of the men and women is cut after the same fashion. It is a loose robe, buttoning with a small robe round the neck, and folding over the breast like a banyan gown, with large long sleeves which cover the hands. People of rank, and especially the ladies, wear several of these gowns

one

one over the other; the undermost one reaches to the ground, the succeeding ones are each shorter than the other, so that the display of the different colours makes a gaudy appearance as they walk along.

Such are the few particulars relative to Cochin China. It now remains to show how a connection with Cochin China may prove beneficial to this country. The drain of specie from the company's settlements in India is become a matter of such serious import, that any plan which may be offered to remedy so growing an evil, I have no doubt, will be deemed worthy of observation. I am sanguine in my expectations, that a settlement in Cochin China would conduce to that desirable end, as well as be productive of many other advantages.

Our two little vessels brought from Cochin China to the amount of 60,000 rupees in gold and silver bullion. The *Rumbold*, the year before, also brought bullion to a considerable amount, on account of sales of Bengal and Madras cloths, opium, iron, copper, lead, hardware, and glass.

The situation of Cochin China is excellently well adapted to commerce. Its vicinity to China, Tonquin, Japan, Cambodia, Siam, the Malay coast, the Philippines, Borneo, the Moluccas, &c. renders the intercourse with all these countries short and easy. The commodious harbours formed on the coast, particularly that of Turon, afford a safe retreat for ships of any burthen during the most tempestuous seasons of the year.

The nations of Europe, having hitherto found it impossible to provide cargoes sufficiently valuable to barter for the commodities of China, are obliged to make up the deficiency by sending thither immense quantities of bullion; by which means it has, for a number of years past, drained the eastern and western worlds of their specie. The number of junks annually resorting to Cochin China, plainly proves how much the productions of it are in demand amongst the Chinese. These productions, had we a settlement and a confirmed influence in the country, might with ease be brought to centre with us, purchased with the staples of India and of Europe. Turon would become the emporium for them, where our ships bound to Canton, from whence it is only five days sail, might call and receive them. It would prove a saving of so much specie to Great Britain or India as the value of the commodities amounted to in China. In a few years, there is every reason to believe a very considerable investment might be provided.

Our trade to China has ever been burthened with enor-

mous imposts and exactions : these, under various pretences, are annually increasing, and in process of time may become insupportable. It is an opinion, latterly grown current, that the Chinese are desirous of totally excluding all Europeans from their country. May we not hazard a conjecture, that the vexations they oblige them to suffer are the premeditated schemes of this politic people to effect it? Were such an event to happen, the want of a settlement to the eastward would be severely felt; the Chinese would export their own commodities, and Java or the Philippines, as the nearest ports, would become the marts for them. As there is no reason to suppose that our inability to procure them from the first land would hinder their consumption, we must buy them either from the Dutch or from the Spaniards. A settlement in Cochin China will give us a superior advantage to either, both as its situation is nearer, and the Chinese are more accustomed to resort thither : at all events there is reason to suppose it will enable us to procure the commodities of China at a much more reasonable rate than now purchased by our factors at Canton, and certainly on less humiliating terms to the nation at large. Colonies of Chinese have from time to time emigrated from the parent country, and fixed their abode in different parts of Cochin China. These have their correspondence in every sea-port of the empire. Through their means, teas, china ware, and the various other articles, the objects of our commerce with China, might be imported in junks to our own settlements, equally good in quality, and cheaper, as the Chinese are exempted from the exorbitant duties levied on foreigners. Some of the best workmen might be encouraged to settle in Cochin China, and, under direction, manufactories carried to as great a degree of perfection as in China itself.

The intercourse between Japan and Cochin China might be renewed, and we might participate in a trade for many years monopolized by the Dutch. An advantageous trade might be carried on with the Philippine islands, and Madras and Bengal goods introduced amongst them, by means of the junks, for the consumption of Spanish America. The Siamese and Cambodians would bring the produce of their respective countries, and barter or sell them for such articles as they wanted from Cochin China. Amongst them it is probable a sale might be found for quantities of Bengal cloths. The lower class of people in Cochin China are, for the most part, clothed in cangas, a coarse cotton cloth brought from China; but the preference, which I had an opportunity of observing, they gave to Bengal cloths, on account of their  
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being wider and cheaper, would soon induce them to adopt the use of them. The demand for opium, already in some measure become a necessary of life to the Chinese, would increase in proportion to the facility of procuring it. The importation of it, no longer confined to Canton, but carried by the junks to every sea-port in the country, would spread the demand for this drug to the remotest parts of the empire.

But what inspires the most flattering hopes from an establishment in this country is its rich gold mines, celebrated for ages as producing the richest ore, so pure, that the simple action of fire is said to be sufficient to refine it. I omitted no opportunity of making inquiries respecting this valuable article, and was told that mines were formed in different parts of the northern provinces, and particularly in Hué, where the ore lay so near the surface of the earth that it was dug up with little labour. Under the direction of a skilful metallurgist, what might not be expected from such a source!

Great as the commercial advantages are, the political ones resulting from a settlement in Cochin China would be scarce inferior. Turon Bay would not only afford a secure retreat to our Indiamen in case of losing their passage to China, but from thence we might also intercept the fleets of any hostile power, either going to or returning from that country. We should become formidable neighbours to the Dutch and to the Spaniards, and, in the event of a war with either of them, attack, with advantage, their most valuable settlements.

Should the company be induced to form a settlement in Cochin China, it may be effected on principles strictly just, and at a small expense. Several of the royal family, besides the mandarins who were in Bengal, with many officers of the late government, urged me to use my endeavours with the government of Bengal to induce it to afford them assistance, promising a powerful support whenever we should heartily join in their cause. To restore their lawful sovereign to the throne, would be now a measure so popular, that the sincerity of their offers cannot be doubted. To relieve an unhappy people, groaning under the weight of the most cruel oppression, would be an act worthy of the British nation. Fifty European infantry, half that number of artillery, and two hundred sepoy, would be sufficient for this and every other purpose. The natives of Cochin China are infinitely below the inhabitants of Hindustan in military knowledge; I have, however, no doubt that a body of them, well disciplined and regularly paid, would prove as faithful to us, and contribute as much to the security of any possessions which we might acquire to the eastward, as the sepoy do to our territories in

India. In case of any distant expeditions, they would be found superior, being entirely free from all religious prejudices, and having no objection to the sea.

While Cochin China remains in its present distracted state, a favourable opening is presented to the first European nation that may attempt to obtain a footing in the country. Should the company, therefore, entertain a design of forming an establishment in Cochin China, no time should be lost in carrying it into execution.

VII. *Report presented to the Class of the Exact Sciences of the Academy of Turin, 15th August 1802, in regard to the Galvanic Experiments made by C. VASSALI-EANDI, GIULIO, and ROSSI, on the 10th and 14th of the same Month, on the Head and Trunk of three Men a short Time after their Decapitation. By C. GIULIO.*

THE First Consul, in a letter to Chaptal, in which he announced to that minister the two prizes he had founded to encourage philosophers to make new researches in regard to Galvanism, says, "Galvanism, in my opinion, will lead to great discoveries." This observation was just and profound: great discoveries have already been made; Galvani and Volta have immortalized their names, and several celebrated philosophers and physiologists have rendered themselves illustrious in this branch of science, so abundant in astonishing phænomena: yet it is only in its infancy, and there can be no doubt that many important discoveries still remain to be made.

Vassali, Rossi, and myself, have for several years been employed in researches on this subject. While the first examined the Galvanic fluid in every point of view, for the purpose of illustrating its nature by means of a great number of ingenious experiments, performed with that care and exactness which are peculiar to him, Rossi and myself attempted to explain the action of the Galvanic fluid on the different organs of the animal economy.

Sometimes I was obliged to interrupt my researches by unfortunate circumstances, and at others by my administrative functions: but I have now resumed them; and though success has not yet crowned my efforts by any brilliant discovery, we trust, and with confidence, that we shall be able to add some valuable facts to the history of the animal economy; to rectify errors; to confirm facts already received; and

to extend the domain of an inexhaustible agent fertile in wonders.

Volta had announced that the involuntary organs, such as the heart, the stomach, the intestines, the bladder and vessels, are insensible to the Galvanic action\*: but we have fully refuted this great physiological error. Unfortunately, however, the Latin memoir containing the decisive experiments which we made on cold-blooded and warm-blooded animals in 1792, presented to the Academy soon after, and which, according to Sue, in his History of Galvanism†, “are curious, and contain very interesting observations,” did not appear till 1801, when it was printed in the last volume of the Transactions of the Academy.

In that interval Grapengieffer found, as we had done, that Galvanism, by means of zinc and silver‡, has an influence on the peristaltic motion. Humboldt ascertained the Galvanic action on the hearts of frogs, lizards, toads, and fishes. Smuch observed the excitability of the heart by the Galvanic fluid; and Fowler changed the pulsations of the heart without the immediate application to it of armatures, and only by adapting them in warm-blooded animals to the recurrent nerve by means of the sympathetic§.

It is chiefly in regard to the experiments of these learned Germans that the historian of Galvanism states||, that the involuntary vermicular motion of the intestines, according to the acknowledgment of all physiologists, obeys metallic irritation; whence it follows, says he, that the Italian philosophers have advanced an error when they said that Galvanism exercises no action but on the muscles, which depend on the will. As an accurate and impartial historian, how can Sue accuse the Italian philosophers indiscriminately of such an error, since he had our memoir before him when employed on the second volume of his History of Galvanism, and since he gave a short account of my experiments in his first volume? Nay, I gave an account of my experiments in a small work published in Italian in 1792. But as Italian works are not much read in France, and were less so at that period, I should not have reproached C. Sue with this act of injustice,

\* Mezzini, Volta, Valli, Klein, Pfaff, Berhends, have denied that the heart could be moved by the Galvanic fluid. *Hist. du Galvanisme*, part i. p. 145. Bichat could obtain no contractions either in the heart of man or that of the dog. See *Récherches Physiologiques sur la Vie et la Mort*.

† Towards the end of the first part.

‡ See *Histoire du Galvanisme*, vol. ii. p. 81.

§ Ibid. vol. ii. p. 84.

|| Vol. ii. p. 83.

and his incorrectness in regard to the Italian philosophers, had not my Latin memoir been known to him, as it had appeared in the Transactions of the Academy.

Though we made a great many experiments before we attempted to combat a philosopher so justly celebrated as Volta, and to establish the influence of Galvanism on the involuntary organs; and though Grapengieffer, Humboldt, Smuch, Fowler, &c. ascertained this influence in certain cold-blooded and even warm-blooded animals; an object of so much importance to physiology required to be extended and confirmed, especially in man, by new experiments. We have been the more sensible of the necessity of establishing this fact in an incontestable manner, either in regard to the involuntary organs in general, or more particularly the heart, as the celebrated Aldini, professor of Bologna, in an Italian work replete with new facts and valuable experiments made on the bodies of decapitated criminals, has been obliged to acknowledge that he was not able to obtain any contraction in that organ by means of the electro-motor of Volta, which is so powerful.

We shall give an account, in particular memoirs, of the experiments we have already made, and of those which we propose to execute. In regard to the stomach, the large and the small intestines, and the bladder, we shall say only, in a general manner, that by armature of the different nervous branches we obtained contractions analogous to those described in regard to animals. The Galvanic action on the heart and arteries is the object of the present paper, as it is of the utmost importance to physiology, and deserves, under every point of view, to excite our attention and occupy our reflections.

Our experiments on the different parts of the head and trunk of the decapitated criminals were begun, on the 10th of August, in a hall of the large hospital of St. John, and resumed and continued yesterday in the anatomical theatre of the university, before a great number of spectators.

We tried the influence of Galvanism on the heart in three different ways:

1st, In arming the spinal marrow by means of a cylinder of lead introduced into the canal of the cervical vertebræ, and then conveying one extremity of a silver arc over the surface of the heart, and the other to the arming of the spinal marrow. The heart of the first individual subjected to our experiments immediately exhibited very visible and very strong contractions. These experiments, as seen, were made with-

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out any intervention of the pile, and without any armature applied to the heart. It is very remarkable, that when the former is touched first, and then the arming and spinal marrow, the contractions of the heart which follow are more instantaneous, and stronger, than when the arming of the spinal marrow is first touched, and then the heart. In a memoir on Galvanism, read in the last public sitting of the academy, I gave an account of a great number of experiments, made especially on frogs, which exhibited a similar phænomenon. In these animals I observed, a great number of times, that when the arming of the crural nerves was touched first, and then the muscles of the thigh, there were no contractions, or the contractions were exceedingly weak; and, on the other hand, that when the muscles of the thighs were first touched, and then the arming of the crural nerves, as long as the least vitality remained in the organs the contractions of the muscles were constant and violent. In the memoir already mentioned I have endeavoured to account for this phænomenon, to which I shall recur when, by a great number of trials, we shall have ascertained that it is as general in men as I found it in frogs and other cold-blooded animals.

The second manner in which we tried the influence of Galvanism on the heart was by arming the nervi vagi and the large sympathetic. The object of these experiments will be readily comprehended by anatomists acquainted with the details of neurology. In these, as well as in the first and other experiments where we armed the cardiac nerves themselves, we obtained contractions in the heart. In this, as in the former case, the contractions obtained when the heart was first touched, and then the arming of the nerves, were much stronger than when the arming of the nerves was touched first, and then the heart. In this method we even observed that the Galvanic experiments sometimes failed.

The third kind of experiments on the heart were performed by means of the pile. The pile we employed on the 10th of August, for the experiments on the first decapitated criminal, was composed of fifty plates of silver and as many of zinc, with pasteboard moistened with a strong solution of muriate of soda. The silver was mixed with a tenth part of copper. This is the proportion which we found most favourable to the intensity of the signs of Galvanism:

	Metre.
The diameter of the silver plates was	0.036
Their thickness	0.0015
The dimensions of the pieces of pasteboard were the same.	
	The

	Metre.
The diameter of the zinc plates was	0.042
Their thickness	0.0035

The pile employed for the experiments on the 15th of August was composed of fifty plates of pure silver, and twice that number of plates of zinc and pieces of pasteboard; the latter moistened in a solution of muriate of soda.

	Metre.
The diameter of the silver plates was	0.038
Their thickness	0.001

The dimensions of the pieces of pasteboard were the same.

	Metre.
The diameter of the zinc plates was	0.04
Their thickness	0.001

By making the negative extremity of the pile to communicate, by means of respective conductors, with the spinal marrow, or merely with the muscles of the back or breast, laid bare, and the positive extremity immediately with the heart, instantaneous and violent contractions were obtained; and the contractions were produced also when the heart was made to communicate with the negative extremity of the pile, and the spinal marrow with the positive extremity.

We shall observe, in regard to contractions of the heart, that of all its parts the apex is the most susceptible of motion, and the most sensible to the Galvanic influence: we must observe also, that the contractions produced by communication with the pile were not only strong, but that they continued a long time even after all the communication was removed.

A very remarkable circumstance is, that the heart, which of all the muscles retains longest, in general, its contractility in regard to mechanical stimulants, is the first to become insensible to the Galvanic influence. The muscles of the arms, and those of the back and breast, continue to be excitable by Galvanism for whole hours; and the heart had lost its excitability about forty minutes after death.

The experiments made yesterday in the anatomical theatre exhibited nearly the same results in regard to the heart as those already mentioned. The great arteries, such as the aorta and some of its branches, being injected with water raised nearly to the same temperament as that of the blood in the living individual, when subjected to the Galvanic action exhibited contractions. But it is probable that they will appear stronger when trials of this kind shall be made on bodies endowed with a higher degree of vitality than those of yesterday, and when the interval between the period of decapitation

decapitation and that of the experiments shall be less. With this view, indeed, we have provided a hall much nearer to the place of execution; for the results which we obtained in the man decapitated on the 10th of August, in which case the experiments were begun five minutes after the decapitation, were all comparatively more striking, and stronger, than those obtained in the experiments of yesterday, which were begun more than twenty minutes after decapitation, and which were performed, as appears, on bodies endowed with a much weaker degree of vitality.

In the experiments made on the arteries, we armed the nervous plexus, which envelop the trunks of the cœliac and mesenteric arteries, several branches of which are even interwoven around the aorta: a communication was established between the positive or negative extremity of the pile and the aortic artery itself. It was by these means that we obtained risible contractions.

If the effects of Galvanism on arterial contractions are constant, as I presume, all those discussions which have been agitated so long, and with so much violence, in regard to the irritability of the arteries, which does not manifest itself by the action of different mechanical and chemical stimulants, will at length be terminated in a positive and irrefragable manner; all doubts will at length be removed; and we shall be indebted to the Galvanic fluid, which is the most energetic of all agents applied to the animal fibre, for having fixed the opinions of physiologists on a point of so much importance to the animal economy.

Whence comes it that Aldini, even with the help of the most powerful electro-motors, was not able to obtain contractions in the heart of man, which we so evidently obtained by the same means which always withstood his efforts? How happens it that we obtained contractions by means much weaker?

The first experiments of Aldini on the human heart were begun an hour and a half after death\*. The trunk had been exposed a long time to the open air, the temperature of which was no more than  $+2$ . It is probable that the cold, and the long interval between the period of death and that of the experiment, had already annihilated the irritability of the heart†. In the fifty-third experiment, the heart of another executed

\* Saggio di Sperienze sul Galvanismo di Gioani Aldini; Bologna 1802, p. 14, esp. 28.

† If the celebrated Bichat failed in his experiments on the human heart, as well as Aldini, it was, perhaps, owing to the same causes. The temperature

executed criminal constantly remained motionless and insensible to the Galvanic current. But in this experiment, before trying the heart, a considerable time was employed in making trials on the voluntary organs, the sensibility of which to Galvanism had already been acknowledged. But the very reverse of this method ought to be followed; for I will here repeat, that excitability, by means of the Galvanic fluid, is extinguished in the heart a long time before it becomes extinct in the voluntary muscles. This is so certain, that while no part of the heart, tried externally and internally, presented any sign of contractions, the diaphragm, and the muscles of the upper and lower extremities, gave very strong ones.

In our experiments which were begun five minutes after death, the heart ceased to be sensible to the Galvanic agent about the fortieth minute; and this was the case in the temperature of  $+25$ ; while the voluntary muscles retained their Galvanic excitability for whole hours. In other experiments made by Aldini, the contractility of the voluntary muscles existed three hours, and even five hours, after death.

In the oxen subjected to Galvanic experiments by Aldini, the excitability of the heart must have been extinguished sooner, since the action of the Galvanic fluid of the pile produced no contractions, though applied immediately after death.

If contractions were observed in the voluntary muscles under the same circumstances, it was because these muscles, which lose much sooner than the heart their excitability in regard to mechanical stimulants, retain it much longer than that organ in regard to the Galvanic agent. What then is the cause of this diversity, which seems contrary to every analogy, and which, however, is proved by facts? It is still involved in much obscurity: but it is not yet time to tear the dark veil which conceals it; we are not yet enlightened by a sufficient number of facts; and the few scattered data which we have been able to collect, cannot yet be connected in a manner capable of encouraging us to attempt to rend the veil at present.

We shall not here speak of the astonishment with which the spectators were struck when they saw the contractions of

perature was cold, and the interval between the time of execution and that of the experiment too long. "I was authorized," says Bichat, "in the winter of the year 7, to make various trials on the bodies of unfortunate persons who had been guillotined. I had them at my disposal from thirty to forty minutes after execution. It was always impossible for me to produce the least motion by arming either the spinal marrow and the heart, or the latter organ and the nerves which it receives from the ganglions by the sympathetic, or from the brain by the par vagum.

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the frontal muscles; those of the eye-lids, the face, the lower jaw, and the tongue; when they beheld the convulsions of the muscles of the arms, the breast, and of the back, which raised the trunk some inches from the table; the contractions of the pectoral muscles, and the exterior and interior intercostal muscles, which diminished the intervals between all the ribs, and made them approach each other with violence, raising the inferior ones towards the superior, and the latter towards the first rib and the clavicle; the contractions of the arms, which, when the uncovered biceps muscle was touched, as well as its tendon, were so speedy and violent, that complete flexion of the fore-arm on the arm took place, and that the hand raised weights of some pounds fifty minutes after decapitation. Similar experiments may be seen in the work of Aldini: our object in this report was merely to speak of the Galvanic influence on the heart and arteries of man, which had not yet been observed.

These new and important results, which we obtained in regard to the heart and arteries of man, will be confirmed by other trials. We shall repeat our experiments as soon as an opportunity occurs, and we shall take care to give you an early account of the most remarkable observations we shall make.

VIII. *A Review of some Experiments which have been supposed to disprove the Materiality of Heat.* By WILLIAM HENRY\*.

THE following remarks on the subject of heat were written soon after the publication of Count Rumford's Inquiry concerning the Source of the Heat evolved by Friction; and of the interesting Essays of Mr. Davy, which appeared in Dr. Beddoes's West Country Contributions. They were transmitted to Dr. Beddoes, for publication, about the close of the year 1799; but circumstances, with which I am unacquainted, have, I believe, induced the doctor to decline the continuation of his periodical work. These circumstances I deem it necessary to state; because, had the essay been written nearer the period of its publication, it would probably have assumed a very different form. At present, I have not leisure to review the subject, or to attempt any material alteration; and still less to examine whether I have been anticipated by any of the authors whose essays have been published during the two last years.

\* From the *Manchester Society's Transactions*, vol. vi. part 2.

*A Review of some Experiments which have been supposed to disprove the Materiality of Heat.*

It has long been a question among philosophers, whether the sensation of heat, and the class of phænomena arising from the same cause, be produced by a peculiar kind of matter, or by motion of the particles of bodies in general. The former of these opinions, though far from being universally admitted, is now most generally received; and the peculiar body, to which the phænomena of heat are referred, has been denominated by M. Lavoisier caloric. Against the doctrine of the French school, some forcible arguments have lately been advanced by Count Rumford and by Mr. Davy, both of whom have adopted that theory respecting heat, which assigns as its cause a motion among the particles of bodies.

The method of reasoning employed by Mr. Davy in proving the immateriality of the cause of heat, is the *reductio ad absurdum*, i. e. the oppugned theory is assumed as true, together with its applications; and facts are adduced directly contradictory of the assumed principles: I shall take the liberty of offering a statement of the argument, rather different from that of Mr. Davy, though, I trust, without misrepresentation, or any material omission.

Let heat be considered as matter; and let it be granted, that the temperature of bodies depends on the presence of uncombined caloric. Now, if the temperature of a body be increased, the free caloric, occasioning that elevation, must proceed from one of two sources: either, 1stly, It may be communicated by surrounding substances; or, 2dly, It may proceed from an internal source, i. e. from a disengagement of what before existed in the body, latent or combined. But the temperature of bodies is uniformly increased by friction and percussion, and necessarily in one of the foregoing modes.

I. Mr. Davy found, by experiment, that a thin metallic plate was heated, by friction in the exhausted receiver of an air-pump, even when the apparatus was insulated, from bodies capable of supplying caloric, by being placed on ice. This experiment he considers as demonstrating, that the evolved caloric could not be communicated by surrounding bodies.

To the inference deduced from this experiment it may be objected, that the mode of insulation was by no means perfect. Admitting the vacuum, produced by the air-pump, to have been complete, still the supply of caloric could not  
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thus be entirely cut off, since it has been shown by Count Rumford that caloric passes even through a torricellian vacuum. If, therefore, friction produce in bodies some change, which enables them to attract caloric from surrounding substances, this attraction may be equally efficient in an exhausted receiver as in one containing an atmosphere of mean density. It would be an interesting subject of experiment to determine the influence of atmospheres of various densities as conductors of caloric; for, since effects are proportionate to their causes, and it is ascertained that common air conducts caloric (better than it is conveyed through a vacuum) as 1000 is to 702, it may be expected that the ratio will hold in all intermediate degrees.

In Count Rumford's masterly experiment, the metal submitted to friction was encompassed by water, and air was carefully excluded from the surfaces in motion: yet the water became hot, and was kept boiling a considerable time. In this case, the only obvious source of caloric, from without, was through the borer employed in producing the friction; if it be true, as the Count has observed, that the water could not, at the same instant, be in the act of giving out and receiving heat. The same objection to the communication of heat, from an external source, exists also in thus explaining Mr. Davy's experiment: but I cannot admit that the argument is demonstrative in proving the evolved caloric not to be derived from external substances; for no absurdity is implied in supposing that a body may be receiving caloric in one state, and giving it out in another. We have an example of the simultaneous admission and extrication of a subtile fluid, the materiality of which is admitted by Mr. Davy, in an excited electric, which, at the very same instant, receives the electric fluid from without, and transfers it to the neighbouring conductors. In an ignited body, also, the two processes of absorption and irradiation of light are, perhaps, taking place at the same moment.

II. Another cause of the increase of temperature in bodies, is the liberation of their combined caloric; and if this be a source of temperature, the absolute quantity of caloric in a body must be diminished by friction. That no such diminution really takes place, we have the evidence of two experiments,—the one of Mr. Davy, the other of Count Rumford. Mr. Davy, by rubbing together two pieces of ice, converted them into water. Now water, *ex hypothesi*, contains more caloric than the ice from which it was formed; and, on the same hypothesis, the absolute quantity of caloric in ice is diminished by friction and liquefaction; which is absurd.

Count

Count Rumford also ascertained that the specific heat of iron was not diminished when converted by a borer into turnings, and consequently when it had been the source of much temperature. In explanation of these facts we may be allowed to assume the communication of caloric from surrounding bodies, till this communication has been demonstrated to be impossible. But even were the impossibility established, it would yet remain to be proved, that the evolved caloric does not proceed from an internal source; and this can only be done by an accurate comparison of the quantity of caloric in bodies before and after friction. Now, in instituting this comparison, it is implied that we possess means of determining the absolute quantity of caloric in bodies, and that we can compare quantities of caloric with as much certainty as we can obtain from an appreciation by weight or by measure. Such perfection, however, does not, I apprehend, belong to the present state of our knowledge respecting heat; for I have always been distrustful of that part of the doctrine which assigns the ratio of heat latent in bodies. The grounds of this distrust I shall state pretty fully; for, if it can be proved that we have no accurate conceptions of quantity, as appertaining to heat, all arguments against its materiality, derived from supposed determinations of its quantity, must be inconclusive.

The only clear conceptions which the mind has of quantity, are derived either from a comparison of the magnitude or of the gravity of bodies. In the instance of caloric, both these modes of mensuration fail us. We cannot estimate the bulk of a substance which eludes our grasp and our vision; nor have we yet succeeded in comparing its gravity with that of the grosser kinds of matter, which it surpasses in tenuity beyond all comparison. Our notions of the quantity of caloric are derived, not from such simple judgments, but from complicated processes of reasoning, in the steps of which, errors fatal to the whole may perhaps sometimes appear.

Whatever be the nature of caloric, whether it be a body *sui generis*, or a quality of other bodies, its effects are peculiar and appropriate; and, like all other effects, bear a proportion to the energy of their cause. Expansion, for example, it is proved by experiment, keeps pace with the actual increments of heat; and on this principle is founded the thermometer, the great agent in the acquirement of all our ideas respecting heat, both absolute and relative. The competency of this instrument, however, to afford information of the quantity of caloric, is limited by the following circumstances:

1st, The mercury of the thermometer indicates only the quantity of heat which it has itself acquired, and by no means

that contained in surrounding bodies. 2dly, The scale of expansion is wholly arbitrary, commencing far from the absolute privation of heat, and falling far short of its maximum. 3dly, The caloric latent in bodies, or chemically combined with them, has no effect on the thermometer. 4thly, The experiments of Dr. Crawford, though sufficient to show that the expansion of the mercury of the thermometer bears a ratio to the actual increments of heat in any temperature between the boiling and freezing points of water, by no means prove that this proportion holds universally.

Equal weights of heterogeneous bodies; it is presumed, contain unequal quantities of caloric; and the ratio of these quantities is approximated in the following manner:

Equal weights of the same body, at different temperatures, give, on admixture, the arithmetical mean; but equal weights of different bodies, at different temperatures, afford a temperature which varies considerably from the mean. Thus a pound of water at  $100^{\circ}$ , and a pound at  $200^{\circ}$ , give the temperature of  $150^{\circ}$ ; but a pound of water at  $200^{\circ}$ , and a pound of mercury at  $100^{\circ}$ , afford, not the mean, but a temperature considerably higher. Hence it follows, that a pound of mercury has not the power of fixing and retaining so much caloric as a pound of water; and the fixation of more heat by the water than by the mercury, is ascribed to the superior energy of a power inherent in both, and termed capacity for caloric.

From an extensive series of experiments Dr. Crawford infers, that the capacities of bodies are permanent so long as they retain their form. Thus, the capacity of water has to that of mercury the ratio of 28 to 1, at any temperature between  $32^{\circ}$  and  $212^{\circ}$ . The difference of capacities of bodies, it is inferred, therefore, would continue the same down to the absolute privation of temperature. Imagine, then, two bodies at this point of privation: they may still contain unequal quantities of combined caloric; for, when chemically combined, caloric does not produce temperature. On Dr. Crawford's hypothesis these comparative quantities of combined caloric in the two bodies may be learned by observing the ratio of temperature produced by the addition to each of similar quantities of heat. This supposition, however, is manifestly gratuitous; and the contrary might be maintained with equal or greater probability; for it may be supposed that at this assumed negation of temperature one body renders latent more caloric than another, because it actually contains less, as certain dry salts attract more water from the atmosphere than others

containing much water of crystallization. The commonly employed mode of ascertaining the specific caloric of bodies is founded, therefore, on an assumption which is deficient in the character of a datum, and which itself requires proof.

If these objections be valid, they will apply also to show the fallacy of the theorem for finding the absolute zero of bodies. By this term some philosophers appear to understand the point of absolute privation of caloric, both free and combined. I apprehend, however, that in strict propriety it can only be used to signify the negation of *uncombined* caloric, or, as Dr. Crawford expresses himself, the point of absolute cold. As applied, however, to water, it is evident that the whole quantity of heat is understood. In ascertaining the zero, say these calculators, the capacity of ice to that of water is as 9 to 10. It is plain, therefore, that when water freezes it must give out 1-10th of its whole heat; and this tenth part is found to answer to 146° of Fahr. Consequently its whole heat is ten times 146, or 1460°; and hence the natural zero is 1460 — 32, or 1428°. Now of this estimate it is a datum, that the capacities of ice and water have precisely the above ratio. But if the general formula for ascertaining the specific caloric of bodies be founded on erroneous principles, it cannot serve as the ground-work of any solid conclusions.

The materiality of caloric may, I apprehend, be maintained, without admitting that we have made any steps towards determining its quantity in bodies; and the arguments of Count Rumford and Mr. Davy are not demonstrative, because they assume that this part of the doctrine of caloric cannot be relinquished without abandoning it *in toto*. I may be permitted, therefore, to state my reasons for believing caloric to be matter; which would have been unnecessary had the contrary been proved with all the force of mathematical demonstration.

Avoiding all metaphysical reasoning on the nature of matter, and assuming the generally received definition as sufficiently characterizing it, I shall examine how far this general character of matter applies to the individual—caloric. Caloric occupies space, or is extended, because it enlarges the dimensions of other bodies; and, for the same reason, it is impenetrable, since, if it could exist at the same time in the same place with other bodies, their volume would never be enlarged by the addition of heat. Of form or figure, as only a mode of extension, it is unnecessary to prove that caloric is possessed; and, indeed, there is perhaps only one general quality of matter that will not be allowed it, viz. attraction. That caloric is influenced by the attraction of gravitation, or

by cohesive attraction, has never yet been proved: yet the various experiments of Buffon, Whitehurst, Fordyce, Pictet, &c. cannot be alleged as proofs that it is actually devoid of this property; since they only decide, that the small quantities which can be artificially collected, are not to be set in the balance against the grosser kinds of matter. One kind of attraction, *that* which has lately been termed chemical affinity, may, I think, after a full survey of phænomena, be fairly predicated of caloric; and if its possession of this quality be rendered probable, we shall thence derive a powerful argument in favour of its materiality.

That chemical affinity has a considerable share in producing the phænomena of heat, appears probable from the following considerations:

1. All the characters distinguishing caloric when separate, cease to be apparent when it has contributed to a change of form in other bodies; and the properties of the substances so changed are also materially altered. Now this is the only unequivocal mark of chemical union that we can apply in any instance; and chemical union implies the existence and efficiency of chemical affinity.

2. The relation of caloric to different substances appears to observe that peculiar law, which, in other instances, is termed elective affinity. If a compound of two or more principles, a metallic oxide for instance, be exposed in a high temperature, the caloric forms a permanent union with the one, but not with the other. In certain instances, caloric is evolved, when two substances, attracting each other more powerfully than they attract caloric, produce, on admixture, an elevation of temperature. In other instances, caloric is absorbed when it is attracted by the new compound, more strongly than by the separate components. Such facts warrant the deduction, that caloric is subject to the laws of chemical affinity. But the precise order of its affinities remains to be decided by future experiments.

3. Caloric seems also, on some occasions, to bear a part in the operation of double elective affinities. In this way it produces decompositions, which, by single affinity, it is incapable of effecting. Thus a most intense fire does not expel entirely the carbonic acid from alkalies: but, when the affinity of an acid for an alkali concurs with that of carbonic acid for caloric, a decomposition ensues. Again: water may be submitted to the highest temperature without imparting a gaseous form to the hydrogen which it contains; but the conspiring affinity of a metal for oxygen occasions the production of hydrogenous gas. On this principle many

chemical facts are resolved into the law of *double affinity*, which are at present explained by that of single elective attraction.

4. Caloric acts sometimes as an intermedium in combining bodies which, without its aid, are not susceptible of combination. Thus carbon and oxygen do not evince any tendency to combination at the ordinary temperature of the atmosphere; but caloric brings them into union, and constitutes itself part of the resulting compound. This, and a variety of other instances, have a striking resemblance to what is called *intermediate affinity*.

In the theory of Dr. Crawford, no influence is allowed to chemical affinity over the phænomena of heat; and, indeed, *that* philosopher expresses a decided opinion that elementary heat is not capable of uniting chemically with bodies. Hence it appears, that the difference between the terms *affinity* and *capacity* is not merely a verbal one, but that they are actually expressive of different powers or causes; and the question, therefore, which of these terms shall be adopted in the description of facts, is one involving the determination of causes.

The term capacity for heat is employed, by Dr. Crawford and others, to denote, in the abstract, that power by which different kinds of matter acquire different quantities of caloric. But in the various applications that are made of this theory, a more precise meaning is often affixed to it; and the term is applied in much the same sense which it has in common language. When thus understood, a difference of capacity necessarily implies a difference in the extent of the spaces between the minute particles of bodies; and that these differences occasion the varieties observed in the acquirement of heat by different bodies. On this theory there is no active principle or power inherent in bodies, and more active in some than in others,—no tendency in the matter of heat to attach itself, in preference, to any one substance. The assigned cause of the phænomena of heat is not, I apprehend, adequate to produce the effects ascribed to it.

On the theory of capacities, a change of form is, in certain instances, antecedent to the absorption of caloric. Thus, when ether is converted into gas, on removing the pressure of the atmosphere, according to this hypothesis, the capacity of the ether is increased by its volatilization; and the change of form is prior to, and the cause of, the absorption of caloric. The order of events, then, in the volatilization of ether is first an alteration of form; next, a change of capacity; and, lastly, an absorption of caloric. On this hypothesis, ether may exist in the state of gas without containing a  
greater

greater absolute quantity of caloric than in a liquid form. But such an interpretation of phænomena is directly contradictory to an established principle, admitted even by those who prefer the doctrine of capacities, viz. that all bodies, during their conversion from a fluid to a vaporous state, absorb caloric. It is at variance also with observed facts: for, if a thermometer be immersed in a portion of ether, confined under the receiver of an air-pump, the temperature of the ether will be found to sink gradually during the exhaustion of the air; and the evaporation becomes proportionally slower, till, at last, it is scarcely perceptible. We may therefore infer, that, at a certain point of diminished temperature, the volatilization of ether would entirely cease, if the supply of caloric from surrounding bodies could be completely intercepted. But, on the theory of capacities, the evaporation should proceed as rapidly at the close as at the commencement of the process; or, in other words, evaporation should be wholly independent of temperature, which every one knows is contrary to fact.

It may be considered, therefore, as extremely probable, that the tendency of ether to assume a gaseous form depends on its chemical affinity for caloric. But, it may be asked, how is this affinity counteracted by an increased pressure, and augmented by a diminished one?

A circumstance absolutely essential to the formation of gases is, that free space shall be allowed for their expansion. Mechanical pressure acts as a counteracting force to this expansion, and either prevents it completely or partially, according to the degree of its application. But from this fact no argument can be drawn against the existence of chemical affinity as an attribute of caloric. Two opposite forces in physics may be so balanced, that neither shall produce its appropriate effect. Thus, a body impelled in contrary directions may remain at rest; yet the operation of the opposing forces, in this case, cannot be denied. Even in chemistry, we have unequivocal examples in which the action of the affinities is suppressed by more powerful causes. Thus, bodies that have a strong chemical affinity are kept perfectly distinct even when placed in contact by the affinity of aggregation. The only inference, then, that can fairly be deduced from the effects of pressure in preventing the formation of gases, is, that it is a power sometimes superior in energy to that of chemical affinity.

Since, therefore, caloric is characterized by all the properties, except gravity, that enter into the definition of matter, we may venture to consider it as a distinct and peculiar body.

Nor is its deficiency of gravity sufficient to exclude it from the class of material substances. Such nicety of arrangement might, with equal propriety, lead us to deny the materiality of light, the gravity of which has never yet been proved; for, besides the experiments of Mr. Michell, which failed in ascertaining this property of light, we have several chemical facts tending to the same conclusion. Thus Mr. Cavendish, after firing a mixture of hydrogenous and oxygenous gases in a close vessel, a process during which much light is always emitted, found not the smallest diminution of weight.

To have completed this defence of the material nature of heat, it would have been proper to have pointed out the circumstances in which the phenomena of heat differ from the known and acknowledged phenomena of motion. At present, however, I have not leisure to pursue the subject at much length; and though several points of disagreement would doubtless be found, I shall mention only one of the most marked and decisive.

Motion is an attribute of matter, independently of which it cannot possibly subsist. If, therefore, the phenomena of heat can be shown to take place where matter is not present, we shall derive from the fact a conclusive argument against that theory of heat which assigns motion as its cause. Now, in the experiment of Count Rumford, before alluded to, heat passed through a torricellian vacuum, in which, it need hardly be observed, nothing could be present to transport or propagate motion. This experiment, in my opinion, decidedly proves that heat can subsist independently of other matter, and consequently of motion; in other words, *that heat is a distinct and peculiar body* \*.

\* The argument at p. 49, which is the basis of my objections to the commonly employed mode of ascertaining specific caloric, I fear, is not so fully and clearly stated as the abstruse nature of the subject requires.

Assuming two bodies, A and B, to be at the point of privation of temperature, or to possess no free caloric whatsoever, the quantity of combined caloric in each, according to Dr. Crawford's theory, is *directly* proportional to the quantities of heat necessary to produce equal elevations of temperature in the two bodies. Thus, if to attain a given temperature, A require caloric as 20, and B only as 10, the combined caloric of A, before this addition, is inferred to have borne to that of B the ratio of 2 to 1. But it might, with equal or perhaps greater probability, have been assumed that the combined caloric of A and B is *inversely* proportional to the quantities of heat required to produce a given temperature; that A, for example, to attain a certain temperature, has absorbed more caloric than B, because in A less caloric existed previously in a state of chemical union,

IX. *Report made to the Philosophical and Mathematical Class of the French National Institute in the Sitting of August 18, entitled A Tour to Upper Egypt above the Cataracts of Sienne, with Observations on the different Kinds of Senna used in Commerce. By C. DESSESSARTZ and VENTENAT.*

**C.** NECTOUX, one of the scientific men whom the government had made choice of to form part of the commission of the sciences and arts which accompanied general Bonaparte to Egypt, being appointed to observe the agricultural system of the country, by examining its plants as well as other objects of natural history, embraced that opportunity of acquiring some more certain information in regard to the senna which Alexandria supplies to all Europe, and particularly to France.

The magazines of Alexandria and Cairo were the first sources which he minutely inspected. All the bales of senna were opened to him, and he found not only the two kinds already known, but also a third plant added to them, the leaves of which have a great resemblance to those of the real senna. But fearing, and with reason, that the information he should thus obtain would be as imperfect and incomplete as that before published, he resolved to examine the different species in the places where they are cultivated, and in the different states of growth, that the description he intended to give might be as full and accurate as possible.

With this view he visited and examined, with great care, the environs of Alexandria, Rosetta, Damietta, and Cairo; but without success: he did not find a single plant of senna in the whole space inclosed in the Delta. The senna, therefore, is called the senna of Alexandria merely because that city is the general entrepôt from which it is transported to Europe; and senna *de la palthe*, because such entrepôts are called *palthe*, which signifies a farm: the managers are called *palthiers*.

By the information obtained from the different palthiers, among whom he mentions, with gratitude, C. Rosetty, and from the inhabitants of the country, he learned that senna plants were to be found in the valleys of Sienne. He therefore proceeded thither, and had the satisfaction of meeting with some, and of collecting several loaded with flowers and fruit. Encouraged by this success, nothing was able to restrain the ardour of his curiosity; neither the violence of the heat, nor the difficulty of a long journey through parched

and rugged mountains, nor the dread of the ferocious Arabs, among whom it was impossible to travel without imminent danger. He dispatched some of the natives, who brought him specimens of the different plants they met with. But being supported by the civil and military authorities, whom the example and wishes of the commander in chief had inspired with the same zeal for the promotion of every thing that could contribute to the progress of the sciences, he penetrated to every place where he had been told or suspected that he should find senna.

It was in the desert in the environs of Bassa-Tine, two leagues from Cairo, that he collected the first plants of senna belledy: on the left bank of the Nile, opposite to Hermantis or Fallach, he met with a greater abundance, as well as in the neighbourhood of Darao.

The good senna and arguel, a kind of cynanchum, of the best quality, and in the highest state of perfection, grow in great abundance in the valley of Basabras, or of Nubia, from which it is brought by the caravans to Darao and Sienne; and it is thence transmitted, at least in the greater part, to Alexandria. In the mountains, three days journey above Sienne, the guebelly, the senna of the Thebaid, and the arguel, are found; the last mentioned in pretty large quantity in a valley which runs to the east of Sienne turning towards Egypt.

In these places, whether valleys, hills, or mountains, the good senna guebelly and the arguel receive no more culture than the belledy, which is considered as wild. The plants grow spontaneously in groups. There are two crops, the abundance of which depends on the duration of the rains, which take place periodically every year. The first, which is the most plentiful, takes place at the end of the rains, which begin at the summer solstice and terminate towards the middle of September. The second takes place about the middle of March.

The preparation consists merely in cutting the plants, and exposing them to the sun on the rocks till they are brought to a state of perfect desiccation. The two kinds are mixed sometimes in Nubia, but the belledy is never found in that part of the country. It is only in the entrepôts of Sienne and Cairo that it is added. The accounts given of the quantities collected and deposited in the magazines, and of those sold, justify the fear of great and dangerous adulteration by the addition of foreign plants. For, according to the acknowledgment of the palthier, the product of the two crops varies from 700 quintals to 1100 or more, a third of which is  
arguel,

arguel, and the sale is 1400 or 1500 quintals. Among that received by our merchants are leaves of the *colutea* and box.

Unfortunately such instances of fraud, which are too common in commerce, are attended with more serious consequences than that practised in regard to various kinds of drugs. They occasion much trouble to our druggists, who are obliged to pick the senna with the utmost care.

The antient botanists distinguished these two kinds of senna, and the terms they employed for that purpose, *foliis acutis* and *foliis obtusis*, expressed with sufficient clearness and precision the differences they exhibit. Linnæus, however, thought it necessary to unite them, and to consider them as varieties. C. Lamarck notices in his dictionary, under the head *cassia*, the error into which this celebrated botanist has fallen. He distinguishes two kinds of senna which grow in Egypt. One, the leaves of which are acute, is announced under the name given to it by Forſkal, *cassia lanceolata*; and the other, the leaves of which are obtuse, under that of *cassia senna*. The observations presented to the class on these two plants by C. Delisle, and afterwards by C. Nectoux, establish also several other differences. The *cassia senna* is distinguished from the *cassia lanceolata*, not only by the obtuse leaves, but also by the stipulæ, which are longer and shaped like a lance, and by its bent pod having on the middle of each face projecting ridges.

The description given by C. Nectoux, when compared with that of C. Delisle, gives a resemblance of characters, which facilitates a distinct knowledge of the classification of these plants, which have been long valued on account of their medical qualities. It is under this point of view we shall consider them.

In the year 6, C. Bouillon-Lagrange published a chemical analysis of the senna of the palthe of Alexandria, which is used in commerce. We conceived that, by delivering to him the three kinds of plants, an ample provision of which had been entrusted to us by C. Nectoux, we should be able to present to the class at the same time a knowledge of the principles contained in the senna and arguel, compared with that already obtained in regard to the senna of commerce. Our wish has been fully gratified by the zeal and ability of C. Lagrange. It will not appear astonishing to you, that the two analyses of the years 6 and 10 present the same results, with the exception of such small differences that they can have no influence on the opinion which physicians ought to form of the action of the two kinds of senna, since the senna  
of

of commerce is nothing else than a compound of the three kinds, which C. Nectoux collected and preserved separately.

The two old kinds were subjected separately to examination, and their products without much impropriety may be ranked in the same class. It remained therefore to subject to the same researches the plant arguel, called senna of Mecca, placed in the class of the cynanchum by C. Nectoux, who has given an ample description of it. The following is the result of the chemical experiments made with it by Bouillon-Lagrange.

“It appeared to me,” says he, “that this substance contained a smaller quantity of extractive matter. Infusion, either cold or hot, decoction, and evaporation of the liquors gave only a small quantity of extract, and this extract in regard to its principles was always analogous to the extracts of the two old kinds of senna. The same effects therefore, but in a weaker degree, are to be expected from it. But this difference is of great importance in the practice of medicine, and ought to be attended to, as well as the form under which the remedy is administered.

The researches made at the same time by our colleague Vauquelin and Bouillon-Lagrange on a constituent part of senna, which has been considered as resinous, and consequently as the most active, will establish more certain ideas respecting this kind of purgative.

C. Nectoux sent with the leaves, small twigs and pods of the senna guebelly, and the flowers which he gathered from that plant. These flowers, according to the researches of Bouillon-Lagrange, give the same results, only that they do not communicate so much colour to the liquor in which they are infused or boiled. They have not so saline and bitter a taste as the leaves; their odour also is different, and every thing announces that they contain less of the purgative principles.

This memoir appears to us particularly important in an economical point of view, as the author proposes to transplant the Egyptian and Nubian senna to St. Domingo, where it may be cultivated in districts abandoned by the planters. C. Nectoux assures us that these plants would succeed equally well in the Isles of France and Reunion, and also in Cayenne.

X. *Biographical Account of MATTHEW BOULTON, Esq.\**

IF genius and indefatigable industry, directed by the purest patriotism, have any claim to the notice of our readers, an authentic account of this gentleman cannot but be highly acceptable to them. When we contemplate the enlarged extent of his views, the wide and rapid circulation of his improvements and discoveries in the most important branches of art, and the numerous and honourable connections which he has formed in every part of the civilized world, we shall be obliged to admit that few men possess greater claims to the attention and gratitude of their country.

Matthew Boulton, son of Matthew Boulton, by Christian, daughter of Mr. Peers, of Chester, was born at Birmingham the 14th of September 1728. He received the chief part of his education at a private grammar-school kept by the Rev. Mr. Ansted, who officiated at St. John's Chapel, Deritend.

So early, we believe, as the year 1745, Mr. Boulton, having lost his father, who left him in flourishing circumstances, distinguished himself by the invention of a new and most ingenious method of inlaying steel. Buckles, watch-chains, and a great variety of other articles wrought at his manufactory, were exported in large quantities to France, where they were eagerly purchased by the English, who affected to have no taste for the productions of their own country.

The confinement of a populous town was but ill suited to such an establishment as soon became necessary for Mr. Boulton's further experiments. Accordingly, in the year 1762, he purchased those extensive tracts of common, at that time a barren heath, with only a small house and mill, on which the Soho manufactory now stands. He laid the foundation of his present extensive works at the expense of nine thousand pounds. To this spot his liberal patronage soon attracted great numbers of ingenious men from all parts; and by their aid he so eminently succeeded in imitating the original, that the most splendid apartments in this and in many foreign countries received their ornaments from Soho. Here, too, the works of the greatest masters in oil colours were mechanically taken off, with such ease and exactness that the original could scarcely be distinguished from the copy. This mode of copying was invented, we believe, by Mr. Egging-

\* From Public Characters of 1800-1801.

ton, whose performances in stained glass have since introduced his name to the public.

The utmost power of the water-mill, which Mr. Boulton had hitherto employed, fell infinitely short, even with the aid of horses, of that immense force which was soon found necessary to the completion of his designs. Recourse was therefore had, about the year 1767, to that *chef-d'œuvre* of human ingenuity, the steam engine. In speaking of that wonderful machine, we shall adopt the animated language of a late excellent Review:—The steam engine, approaching to the nature of a perpetuum mobile, or rather an *animal*, is incapable of lassitude or sensation, produces coals, works metals, moves machines, and is certainly the noblest *drudge* that was ever employed by the hand of art. Thus we “put a hook in the nose of the Leviathan:” thus we “play with him as a child, and take him for a servant for ever\*,” thus “we subdue nature, and derive aid and comfort from the elements of earthquakes†.”

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The first engine that Mr. Boulton constructed was on M. Savary's plan, of which the reader will find one of the most satisfactory accounts in Professor Bradley's “New Improvements of Planting and Gardening‡,” &c. But the machine was yet, as it were, in its infancy, and by no means answered Mr. Boulton's expectations. In the year 1769 Mr. James Watt, of Glasgow, obtained a patent for such a prodigious improvement of it, that Mr. Boulton immediately sought his acquaintance, and induced him to settle at Soho. At this place, the facility of its application to a variety of concerns, wherein great force was requisite, soon manifested its superior utility and vast advantages to the public: Parliament, therefore, in 1775, cheerfully granted a prolongation of Mr. Watt's patent for twenty-five years. A partnership now commenced between Messrs. Boulton and Watt; and a manufactory of steam-engines, on their improved plan, was established at Soho, which still supplies the chief mines and manufactories throughout the kingdom.

Aided by such talents, and commanding such unlimited mechanical powers, Mr. Boulton's views soon expanded, and Soho began to exhibit symptoms of the extraordinary advantages it had acquired. The art of coining had long stood in need of simplification and arrangement; and to this art Mr. Boulton no sooner turned his attention, than, about the year 1788, he erected a coining-mill on an improved plan,

\* Job, xli. 2—4.

† Analytical Review, Feb. 1797, p. 220.

‡ Seventh edit. p. 315.

and struck a gold medal of the full weight of a guinea, and of the same form as that of his new copper coinage lately put into circulation. The superior advantages of that form are obvious. The impression is far less liable to friction; and by means of a steel gauge of equal diameter, money coined on that principle may be examined by measure as well as by weight, the rim being exactly circular. Moreover, the intrinsic is so nearly equal to the current value of every piece, that, without a steam-engine and adequate apparatus, every attempt to counterfeit the Soho coinage must be made with loss. The fabrication of base money seems likely, by these means, to be speedily checked, and, it is to be hoped, entirely defeated. The reason why Mr. Boulton has not yet been employed by government in the coinage of gold and silver, we have not been able to learn.

The mill at Soho works eight machines, *each of which receives, stamps, and delivers out, by the aid of only a little boy, from seventy to ninety pieces of copper in one minute.* Either of them is stopped without the smallest interruption to the motion of the others. In adjoining apartments all the preparatory processes are carried on with equal facility and dispatch; such as rolling the copper into sheets, dividing them into blanks, and shaking them into bags clean and ready for the die. Without any personal communication between the different classes of workmen, &c. the blanks are conveyed to the room where they are shaken, and from thence to the coining-room, in boxes moving with immense velocity on an inclined plane, and accompanied by a ticket of their weight.

The Sierra Leone company have employed Mr. Boulton's mint in the coinage of silver, and the East India company in that of copper. Two complete mints have likewise been lately sent to Petersburg.

Since the demise of the late empress Catharine, Mr. Boulton presented her successor, the late emperor Paul I., with some of the most curious articles of his manufactory, and in return received a polite letter of thanks and approbation, together with a splendid collection of medals, minerals from Siberia, and specimens of all the modern money of Russia. Among the medals, which for elegance of design and beauty of execution have never yet been equalled in this or any other country, is a massy one of gold, impressed with a striking likeness, it is said, of that monarch. Our readers will be surprised, when they are told that this unrivalled piece was struck from a die engraved by the present empress dowager,

ager, who has from her youth taken great delight in the art of engraving on steel.

With the view of still further improving and facilitating the manufactory of steam-engines, Messrs. Boulton and Watt have lately, in conjunction with their sons, established a foundery at Smethwick, a short distance from Soho. Here that powerful agent is employed, as it were, to multiply itself, and its various parts are fabricated and adapted together with the same regularity, neatness, and expedition, which distinguish all the operations of their manufactory. Those engines are afterwards distributed to all parts of the kingdom by the Birmingham canal, which communicates with a wet dock belonging to the foundery.

To such amazing perfection has the steam-engine at length been brought, that the consumption of one bushel of Newcastle coals will raise nearly six thousand hogsheads of water ten feet high, and will do the work of ten horses for one hour. This remarkable abridgement of human labour, and proportionate diminution of expense, are, in a great measure, the result of trials made under the auspices of Mr. Boulton. But for a more complete account of these machines, their power, &c. we must refer the reader to Dr. Darwin's Botanic Garden\*.

It could scarcely be expected that envy would view with indifference such singular merit, and such unexampled success. The inventions and improvements of Messrs. Boulton and Watt were first imitated, and then either decried or disputed. Reason laboured in vain to silence the clamours of injustice, and to defeat the stratagems of fraud. At length, in the year 1792, a solemn decision of parliament, and, about the same time, the concurrent opinion of the court of king's bench, forbade any further encroachment.

Whoever contemplates the merit and utility of a long life devoted to such valuable pursuits, as we have here briefly and very imperfectly described, and recollects without emotion, that the spot whereon so much has been done, and is still doing; where hundreds of women and children easily earn a comfortable subsistence†; where population is rapidly increasing, and the means of national prosperity increasing in

\* Fourth edit. note xi. page 287.

† We have been unable to ascertain the number of hands employed by Mr. Boulton at this time, which must frequently vary according to the changes that necessarily take place in the demand for different articles; but we know, that when Mr. Boulton junior came of age, in 1791, seven hundred workmen sat down to an entertainment given by his father.

proportion, was lately a bleak, swampy, and sterile waste, must want understanding to comprehend, or sympathy to appreciate, the happiness of his fellow-creatures.

Mr. Boulton is now in his seventy-third year, and he appears to possess the hilarity of youth. Extraordinary exertions, often both of body and mind, seem not to have impaired a constitution which must have been naturally robust. He is fond of music, and takes great delight in the company of young people. One son, a young man of considerable accomplishment and great promise in his father's line, and one daughter, both of them unmarried, have survived their mother. Mr. Boulton is fellow of the Royal Societies of London and Edinburgh, and of the Free Economical Society of Petersburg, as well as of many other foreign institutions.

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XI. *On the Effects of Elder in preserving Plants from Insects and Flies.* By M. CHRISTOPHE GULLET \*.

COMMON elder has appeared to me useful, 1st, for preventing cabbage plants from being devoured or damaged by caterpillars: 2d, to prevent blights, and their effects on fruit- and other trees: 3d, to preserve corn from yellow flies and other insects: 4th, to secure turnips from the ravage of flies, &c.

1st, The strong and fetid odour of a bunch of elder leaves induced me to think that different kinds of butterflies might be incommoded by it in proportion to their delicacy. I therefore took some young twigs of elder, at the period when butterflies began to appear, and whipped well with them some cabbage plants, but in such a manner as not to damage them. Since that time, during two summers, though the butterflies hovered round the plants, I never saw one of them settle on them; and I do not think that a single butterfly was hatched on the cabbages treated in this manner, though a neighbouring board was dirtied by them in the usual manner.

2d, After a short reflection on the effects here mentioned, and on blights, which, in my opinion, are chiefly occasioned by small flies and small insects whose organs are still more delicate than those of the former, I was induced to whip in the same manner with elder twigs, as high as I could reach, the branches of a plum-tree which grew in an espalier. The

\* From the *Bibliothèque Physico-économique*, Ann. 6:

whipped

whipped leaves remained green and in a good condition, while from at least six inches above to the top of the tree the rest of the leaves were blighted, wrinkled, and full of worms. It is here to be observed that the tree was in full flower when I whipped it, therefore much too late for this operation, which ought to have been performed once or twice before flowering. But I am of opinion, that if trees were besprinkled with a strong infusion of elder every eight or fifteen days, the success would be certain, and that there would be no danger of injuring either the flowers or the fruit.

3d, What the farmers call the yellows in corn, and which they consider as a kind of blight, is the effect, as every one knows, of a small yellow fly with blue wings nearly of the size of a gnat. It lays its eggs in the ear of wheat, and produces a worm almost invisible to the naked eye, but which, when seen by a magnifying glass, is a large yellow larva, having the shining colour of amber. This fly is so productive, that I have counted upwards of forty worms in the chaff of one ear of wheat, which was a number sufficient to destroy it entirely. I therefore proposed to make my experiment as soon as possible; but the heat and drought of the season having advanced the wheat more than usual, it was in flower before I could attempt it. Next morning, however, at break of day, two servants having drawn bundles of elder over the ears of wheat on each side of the furrow, backwards and forwards, in places where the wheat was not so far advanced, I hoped that the fetid effluvia of the elder would prevent the flies from remaining on the ears that were covered with them: and, indeed, I was not entirely disappointed; for, on examining my wheat some time after, I found that the part which had been beaten with elder was much less damaged than that which had not been treated in the same manner. I have no doubt, that, had I employed this precaution sooner, the corn would have been completely preserved. Should this be the case, the process is simple; and I flatter myself that fine crops of corn may be saved by these means from this small insect, which is so destructive to them. One of these yellow flies laid on my thumb at least eight or ten eggs, of an oblong form, in the small interval of time which I employed in walking over two or three furrows, holding it by the wings, and which I could not observe without the assistance of a magnifying glass.

4th, It often happens, that whole crops of turnips are destroyed while young, in consequence of being pricked by certain insects. I have great reason to think that this evil may be prevented in an effectual manner, by causing a person to draw a bunch of elder, sufficiently large to cover about the breadth

breadth of a foot, over the young turnips, going backwards and forwards. What confirms me in this idea is, that, having drawn a bunch of elder over a bed of young cauliflowers which had begun to be pricked, they afterwards remained untouched by these insects.

Another fact which tends to support this idea is, that when my neighbourhood, about eight or nine years ago, was so infested with caterpillars that they devoured all the vegetables, leaving scarcely a green leaf untouched, they spared the elder-trees amidst this general devastation, and never molested them. In reflecting on these circumstances, I am of opinion that the elder might be introduced with advantage into our gardens, as the means of preserving fruit-trees and various plants from the rapacity of insects.

The dwarf elder appears to me to exhale a much more fetid smell than the common elder, and therefore ought to be preferred in making experiments on this subject.

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XII. *Reply to a Charge of Plagiarism, brought by Mr. HENRY CLARKE against W. DICKSON, LL. D. Communicated in a Letter from Dr. DICKSON to Mr. TILLOCH.*

I HAVE partly drawn up, and hoped before this to have laid before the Public, a Defense of Sir Isaac Newton, and some other distinguished British mathematicians, against the misrepresentations of Mr. Henry Clarke, and of the writer of the mathematical articles of the Monthly Review. But a distressing complaint, which is alleviated but not yet removed, has, as you know, long rendered me almost, and at times altogether, incapable of the necessary application to more serious concerns, which will demand my whole attention for some time to come.

I certainly owe the gentlemen some return for their Animadversions\* on one of my notes on C. Carnot's excellent piece on the Infinitesimal Calculus; and it will be easy to repay them in their own coin, and with interest. In that instance, it is true, and perhaps in others, I unfortunately

\* See Animadversions on Dickson's Translation of C. Carnot's Reflections on the Theory of the Infinitesimal Calculus, by Henry Clarke, late *Prælect. Coll. Mancun.* &c. sold by T. Hurst, No. 32, Paternoster Row.—Also the Monthly Review Enlarged, vol. xxxviii. p. 440, 441. N. Series.

mistook the sense of my very acute and ingenious author ; for which I am ready to make *him* and my readers any reasonable apology\*. But it is also true, as Mr. Clarke himself confesses, that I “ have endeavoured to do justice to my author,” and, “ on the whole, have properly appreciated his merit.” Yet it is no less apparent, that both Mr. Clarke and the Monthly Critic, not contented, as the Critical Reviewer has been, with correcting my error, have, *without the smallest provocation*, treated me with the greatest harshness and contempt. Their motives will appear hereafter, from the internal evidence of their own writings, compared with my notes on C. Carnot ; and from strong collateral circumstances. Among other things, it will appear, that Mr. Clarke, as well as the Monthly Censor, is in some degree, a critic by trade ; that both are in the habit of emitting their decisions, with an air of despotic authority, which ill becomes men whose abilities, considerable as they are, cannot hide their infirmities, far less justify their insolence ; and that, in many instances, they ought rather to implore pardon, than to provoke recrimination. Mr. Clarke has already been taxed with “ *nonsense*” in correcting my error† ; and the “ injurious assertions” of the Monthly Critic have lately obliged the justly celebrated Dr. Hutton, of Woolwich, to drag the “ concealed tyrant” out of his hole, and to expose him publicly *by name*.

I shall be justified by the evidence before me, to consider my censors (especially the Monthly one) in certain *other* lights than those of self-conceited and unjust critics, and illogical, new-fangled, mathematicians.—But my chief object at present is, to repel a charge of plagiarism, brought against me by Mr. Clarke, in these words :

“ P. S. The Translator informs the reader, in his note, p. 47, that the general method there shown of finding the fluxions of quantities, is “ *in some measure new*.” But if the reader will look into *Clarke's Rationale of Circulating Numbers, with useful Remarks on various Parts of the Mathematics*, published about twenty years ago, he will find the very same process adopted, and the application thereof copiously exemplified !”

Such is the charge which Mr. Clarke, no doubt to make it conspicuous, has placed in a Postscript after “ the end” of his Animadversions on my Translation of Carnot ; and has finished it with a ( ! ) by way of a sting to its tail.

\* My error cannot embarrass the reader. He has only to omit my note, p. 26.

† In the Critical Review, vol. xxxiv. New Arr. p. 359.

In the first place, I solemnly declare that this charge is false and groundless in every possible respect.

*Secondly*, I deny that I have there “shown a general method of finding the fluxions of quantities.” I have only inserted in a note, “a specimen”—“a slight and imperfect general outline”—of a very easy, and in some measure new, manner of treating the fundamental processes of fluxions, which I long ago mentioned to yourself; and which, with some thoughts on Prime and Ultimate Ratios, I intended to offer you for publication,” &c.\* The substance of the paper here alluded to, which you may recollect my mentioning, I submitted to a certain Gentleman whose education does honour to the University of Cambridge. But the paper itself, owing to my indisposition and avocations, is not yet finished; and, owing to the treatment I have received, is likely never to be printed, any more than some analogous pieces of more consequence, which I intended for the press. This, Mr. Clarke will say, perhaps very truly, is no loss to the public. But unless he can divine the contents of a piece, which has no public existence, his charge of plagiarism must become impotent, or can only affect it's fabricator.

*Thirdly*, Whether even the “specimen” of the method which I *promised*, be, or be not, “the very same” with Mr. Clarke's, is a matter of fact, of which those persons who compare them can judge; and such I leave to determine. Whether they know any book from which that “specimen” differs so widely as it does from that of Mr. Clarke.

I pretended not to a method entirely, or even in a great measure, new. My principal object was *facility*, which is but too much neglected by great mathematicians. I thought, however, that my first way of finding the fluxion of a fraction was my own, as I still think the second is. Certainly, I never received either of them from any man, or from any book. The first is as follows:

“The fluxion of a fraction may be found by considering it as the product of the numerator and denominator, giving the latter a negative index. Thus  $\frac{x}{y}$  is equivalent to  $y^{-1} \times x$ ; and, as the fluxion of  $xy$  is  $y\dot{x} + x\dot{y}$ , so the fluxion of  $\frac{x}{y}$ , or of its equivalent  $y^{-1} \times x$ , must be

$$\left(y^{-1} \times \dot{x}\right) + \left(x \times -y^{-2} \dot{y}\right) = \frac{\dot{x}}{y} - \frac{x\dot{y}}{y^2} = \frac{y^2\dot{x} - yx\dot{y}}{y^3} = \frac{y\dot{x} - x\dot{y}}{y^2}.$$

\* See my Translation of Carnot, notes p. 48 and 49.

This process, I say, I took to be my own. But since Mr. Clarke has rudely forced me to look into his obscure book, and a great number of better ones which I never before consulted, I find it in the *table*, in which, as he says, he has “copiously exemplified” his method; whether intelligibly or not, to beginners, whom he professes to instruct, it is not my present business to inquire.

In Mr. Clarke's first column, marked “Variable Quantities,” stands “ $\frac{x}{y} = xy^{-1}$ ,” and in his second column marked

“Fluxions,  $x \times -y^{-2} \dot{y} + y^{-1} \dot{x} = \frac{y\dot{x} - x\dot{y}}{y^2}$ ,”

Mr. Jones, I find, states the same process, thus,

“F,  $\frac{x}{y} \left( x \times \frac{1}{y} \right)$  is  $\left( \dot{x} \times \frac{1}{y} - \frac{\dot{y}}{y^2} \times x \right) \frac{y\dot{x} - x\dot{y}}{yy}$ ,”\*

And Mr. Holliday thus: “What is the fluxion of  $\frac{x}{y}$ , or  $xy^{-1}$ ?—1st,  $\dot{x} \times y = y\dot{x}$ ; and 2d,  $\dot{y} \times x = x\dot{y}$ : then

$\frac{y\dot{x} - x\dot{y}}{yy}$  the fluxion. For  $\frac{\dot{x}}{y}$  or  $\dot{x} \times \frac{1}{y}$  is  $\dot{x} \times \frac{1}{y} - \frac{\dot{y}}{yy} \times x = \frac{y\dot{x} - x\dot{y}}{yy}$ .”†

Now, have not I as good a right to say (what however, I am very far from saying) that Mr. Clarke copied from Jones or Holliday, as he has to affirm, that I copied from him? And, is it not a presumption in my favour, that I have been more explicit than any of them; although I only offer a “specimen” in a *note*, and they profess, as Mr. Clarke expresses it, to render the principles of Fluxions “clear to the lowest capacity ‡?”

Be this as it may, in the same note, I mention a second, and still easier, way of doing the same thing, in these words,

“The fluxion of a fraction,  $\frac{x}{y}$ , may also be found, but not so elegantly, by actually dividing  $x + \dot{x}$  by  $y + \dot{y}$ .” Of this,

\* Synopsis Palmariorum Matheseos, printed in 1706, p. 229.

† Introduction to Fluxions, printed in 1777, p. 96.

‡ See Mr. Clarke's Preface, p. xii. Jones and Holliday make the same profession in their title-pages and prefaces; and the latter appears, in general, to have fulfilled it.

which

which indeed has little to recommend it but it's obvious facility, not being able to find a trace in any author, I may fairly claim it as my own; and, if so, may now affirm (what I have not before affirmed) that even my "slight and imperfect specimen," and, *a fortiori*, the *promised* method itself, is, "in some measure new"—the point to be proved.

I remain, Dear Sir,

Truly and respectfully yours,

WILLIAM DICKSON.

P. S. My language may be thought somewhat severe. But those who read the critiques of which I complain, must acknowledge, that it is more than justified by the provocation. To such personal provocation, I have hitherto been an *utter stranger*; though, you know, that for six years, I laboured to promote, by all the fair, honest and legal means in my power, the Abolition of the nefarious African Slave-trade. I do not even except the unprovoked piece of invective which first contributed to "drag me into that controversy"—one of the most angry, and voluminous, which ever agitated this nation.—In what I have farther to write, I shall endeavour to preserve better humour, and to return "dry rubs," rather than dry blows,

"To words far bitterer than wormwood,

"Which would, in Job or Grizzel, stir mood."

The ends of self-defence (not to mention here a certain more important end) will be abundantly answered by a defence of the illustrious dead, whose ashes, as is known to you and others, I have long wished to see protected by some abler hand, from the profanation of the Monthly Reviewer, in the licentious exercise of his *function*.—What I mean to publish, shall be *conclusive* on my part. Controversy, though I suffered no discredit in the important one just mentioned, has no charms for me; nor shall I contend, farther than is absolutely necessary, with men who, for aught that appears, watch for opportunities, and will not be nice about the means, of exalting themselves, at the expense of others.

W. D.

\* See my Letters on Slavery, p. 4.

XIII. *A general View of the Coal-Mines worked in France, of their different Products, and the Means of circulating them.*  
 By C. LEFEBVRE, Member of the Council of Mines, of the Philomatic Society, &c. &c.\*

I HAD proposed for several years to exhibit a view of the resources of France in regard to combustible fossils. I was desirous of making known, at the same time, the principal places of consumption, the grand metallurgic establishments, where these substances are, or might be, employed; and to add to this labour an indication of the localities proper for the creation of new manufactories, according to the union presented by nature of mineral substances, capable of being worked, and of abundance of fuel.

It appeared to me also, that, to fill up this sketch, it would be necessary to include pretty certain data; in regard to the nature and quantity of the resources which the forests and woods might furnish for consumption in different parts of the French territory, and particularly in regard to those most interesting to be considered under the view of large manufactories †.

The information collected by the council of mines for seven or eight years, furnished a variety of details and interesting facts in regard to the mineral substances known and dug up in our territory. The notes respecting coal mines in particular were the most numerous, because the administration of mines, feeling the necessity of supplying the diminution of the products of our forests, and the importance of ensuring constant activity to our manufactories, by means of coals, had paid the most serious attention to this object.

A great many indications, however, required further con-

\* From *Journal des Mines*, No. 71.

† A good map of the communications, both by land and by water, pointing out the forests, coal mines, the turf mosses, and the large manufactories, would be of great value to commerce. It would certainly contribute to give more activity to internal commerce. A map of this kind is wanted, and the construction of it appears to be an object worthy the attention of the present government.

The council of mines sketched out, in the year 4, a part of this operation on the navigation chart by Dupain-Triel. It contained marks which pointed out the position of the coal mines, turf mosses, forges, great founderies, salt works, and some other large manufactories, respecting which accurate information had been obtained; but this labour is now very imperfect, because the new departments are not included in this chart, which, on account of the new information obtained since that period, ought to be added.

firmation, and whole districts of France, which were supposed to be highly interesting, were still imperfectly known in regard to mineralogy. The council had not been able for several years to send out many engineers to explore the country; many of the departments earnestly requested that researches might be made in this manner; and the distribution of these functionaries in the places where their presence was so necessary, could not fail to be one of the first measures of a government whose attention was so zealously occupied with the prosperity of the state\*.

As I had the strongest reason to hope that the results of the researches and observations of the engineers of mines would furnish the most valuable materials for the proposed view of our mineral resources, I resolved to wait until their residence in the different departments should have enriched the administration of mines with new information proper for rendering more perfect my proposed labour.

I was besides persuaded that, during that period, the solicitude and enlightened alacrity of the magistrates who compose the general administration of forests, would give more extent and correctness to our knowledge respecting the consistence and products of our woods; that, perhaps, we should even have a map of the forests; and that the work which I projected might still be completed in this very important department.

I confined myself, therefore, to the collecting of notes; to verifying as far as possible, and to extending the first data in the possession of the administration of mines, and to making researches in regard to the existing means of circulation, and those which it would be proper to create, in order to take as much advantage as possible of the materials which were the object of my labour.

But at the moment when peace, restored to Europe, inspired a desire of renewing our commercial intercourse with the neighbouring nations, we endeavoured to examine what really were our wants, and what advantages would arise to us from the reciprocal exchanges which might take place.

Our attention was directed in particular to mineral substances: we knew that before the war they had been a considerable object of importation: coals in particular had been

\* In consequence of measures taken this year (an. 10) by Chaptal, the minister of the interior, sixty departments are to be visited and carefully examined by engineers, who will concur with the præfects in the improvements to be made in those parts, and who will give an account of them to government. This is all that could be done with the present number of engineers who can be spared for that purpose.

brought in great abundance into our territory, though the use of that kind of fuel was less frequent at that period than at present, and much less, no doubt, than it will become in consequence of the deterioration of the wood. It was, therefore, of more importance than ever to appreciate properly our new situation in this respect, after our increase of territory, the fruit of the conquests of our armies. It was necessary to determine whether we ought to admit among us the products of foreign coal mines; considering on one hand the quantity of money exported,, and the decrease or even stagnation of industry which might ensue in our national mines; and on the other, whatever was proper for the security and improvement of our manufactures.

All the data proper for the solution of this question were subjected to government. The different opinions pro and contra were defended with that warmth which is always excited by objects of great national importance, among men strongly attached to their country, and deeply affected by every thing which ought to have an influence on their destiny.

Whatever may be the determination of government, we must rely with confidence on the wisdom of its views, and be assured, that it will not decide without being fully informed on the subject.

In publishing at present this view of the resources presented by our coal mines, I have no intention of entering into a discussion of the question already mentioned: but several persons having urged me to make known, as far as possible, our riches in this department, I thought that by complying with their request I should render my labour important in several points of view; first, as supplying information useful to statesmen, who might be desirous of details on this subject, and to the different consumers and to the workers of mines.

I have followed the alphabetical order of the departments; each of them has been reviewed; and from the information obtained I have enumerated all the known mines now worked, those susceptible of being worked, and those indications not yet verified which seem to deserve an attentive examination.

I have exhibited as far as possible the mass of the productions of the coal mines in each department where they are worked, and the mean price of the coals, both at the mines and in the principal places of consumption.

I must however observe, that there are some departments where the mines are worked in so irregular a manner, that it was impossible for me to obtain data sufficiently satisfactory,  
either

either in regard to the quantity of coals extracted, or to their value. In that case I have only made known the districts where the coals are found.

To the means of circulation which exist, or those which ought to be established, I have paid particular attention. I have indicated these means as far as possible; and under this point of view in particular I hope my labour may be useful, because, by making known the means of circulation, the public attention may be directed to the transportation and use of our combustible fossils, in places where it has hitherto been supposed that they could not be employed with advantage. With this view I have added to this work a chart, where the means of circulation may be traced out, by corresponding numbers placed in the margin, and by lines drawn or dotted with red, which mark the existing modes of circulation, and those projected, or which it may appear of advantage to establish.

The departments in which no fossil combustibles are dug up have nevertheless been presented in their order; and I have announced whence these substances may be extracted, and the means by which they may be transported.

I have exhibited also a general view of the various considerations in regard to public advantage and economy which result from the present state, as far as it is known, of our resources of mineral fuel.

#### *Department of Ain.*

This department exhibits no strata of coals worked at present. At Surjoux, in the canton of Seyssel, on the banks of the Rhine, asphaltes or mineral bitumen is found. It is extracted from beds of coarse granitic freestone, analogous to that which covers certain strata of coal, or which is found in alternate strata with it. Several banks of this bituminous freestone are found all round the commune of Surjoux. They have very little consistence, on account of the abundance of liquid bituminous matter with which they are mixed.

C. Suretan, an inhabitant of Seyssel, who has a grant of a part of those districts, extracts and prepares this mineral bitumen. The products extracted amount annually to about ten thousand myriagrammes. This substance may be employed for daubing over cordage and wood, to defend them from water and from the attack of worms. It is useful also for greasing the axle-trees of carriages, and different parts of machines, to facilitate their rolling.

The means of circulation of these productions are the

Rhone descending towards Lyons, the South of France, and particularly the sea-ports in that quarter, where the use of this bitumen may be attended with economical advantage.

*Department of Aisne.*

This country contains no coal mines (1) \* worked at present: a substance known under the name of *coal earth* is commonly found in it, forming beds of greater or less thickness, and generally placed at no great depth beneath the cultivated lands. It is a sort of exceedingly pyritous turf. It burns badly, and cannot be employed for the same purposes as real coal: it would be very bad for heating apartments, or for working iron. A great deal of this substance, however, is dug up; it is employed by the farmers, who spread it out, either in its natural state, or after it has been left to waste in the open air, on the fields, which it renders fitter for vegetation.

The abundance of pyrites dispersed throughout the strata of turf render them susceptible, perhaps, of yielding, by proper treatment, sulphate of iron (green copperas). The coal pits of the northern departments will easily furnish coal for the consumption of that of Aisne, when the junction canal between the Scheldt and the river Oise shall be terminated.

*Department of Allier.*

There are coal mines worked at Noyant, (2) in the commune of the same name, situated six leagues south-west of Moulins, on the edge of the road from that town to Montluçon. At the distance of three leagues further, proceeding towards the latter place, there is another coal mine now worked; namely, that of Fins, in the commune of Châtillon; and a league and a half further, but on the other side of the road, a third, that of des Gabliers, in the commune of Tronget.

The coal hitherto extracted at Noyant is that proper for using under boilers, and for other purposes of a similar kind. That furnished by the mines of Fins and Gabliers is for the most part of an excellent quality, and proper for forges. The annual product of these mines is about a million of myriagrammes; but they are far from being carried to that degree of activity of which they seem to be susceptible. It is announced that measures are about to be adopted in regard to the mine of Fins, calculated to obtain from it all the advantages which the depth of the strata and the valuable quality

\* These numbers refer to the map.

of the coals seem to promise. The product of these mines is consumed in the neighbouring glass-houses, and the coals are transported by water down the Allier, on the Loire, the canal of Briare, and the Seine.

A plan has been long in agitation to employ the small river of Quenne to facilitate the transportation of these coals as far as the Allier. If this project be susceptible of execution, there can be no doubt that this mode of communication with the river Allier will be of great importance; for the expense of land-carriage as far as Moulins already doubles the price of the coal. It varies in these mines from six to ten per cent. the myriagramme: it has already risen at Moulins from twelve to twenty per cent.; and when delivered at Paris from twenty-four to forty per cent.

This department exhibits also, in the commune of Commentry, considerable strata of coal of excellent quality, and might be worked with advantage, if a consumption be created for them: this at present is absolutely wanting, and for this reason these mines are very little worked. The engineers of mines who have visited this country have pointed out several new strata not before observed.

The mines of Plaveret and de Bouije furnished in the year 3, a hundred thousand myriagrammes, which is far below the quantity that might easily be extracted. The price at the mine is about four cents per myriagramme.

If the Cher were rendered navigable from Montluçon to Vierzon, where it is so at present, the mines in the neighbourhood of Commentry would soon give rise to enterprises which would enliven that country. It is probable that they would have a very happy influence on the activity of the numerous forges situated on the borders of the Cher, and at a small distance from it, by the application of a fuel so strong as coal to a part of the operations of these forges.

There are indications of coal in several parts of this country which deserve to be examined, if there were easier means of disposing of them, or if consumption could be found for them on the spot. Such are the indications found by C. Rambourg, near the forest of Tronçais, in the canton of Meaulne; and those in the commune of Vallon, in the same canton, discovered by Thiebault of Allier.

#### *Department of the Lower Alps.*

Some coal mines (3) are worked in the neighbourhood of Manosque and Forcalquier. They are worked very irregularly. The products are not well known. The quality of the coals is very indifferent. They are sold for about 20 cents

cents per myriagramme, delivered at the mine. There are very few means of disposing of them.

*Department of the Upper Alps.*

Coal mines are found (4) in the commune of Saint Martin de Querieres, and in the neighbourhood: the same observations may be applied to these coal mines as have already been made in regard to those of the Lower Alps. They have however a more certain and an easier sale, in consequence of the neighbourhood of the town of Briançon, where the consumption is very considerable, as wood is exceedingly scarce in that canton.

*Department of the Maritime Alps.*

A grant was passed in the year 9, of the coal mine (5) in the neighbourhood of Roquebrune. The grantee has lately announced that the first attempts were not successful. It, however, appears that some of this fuel is dug up in several places in the neighbourhood of Monaco. These mines might acquire a certain degree of importance, if the coals were of a good quality, and if they were exported through the port of Monaco, or if manufactories of iron articles were established in that department, or in the environs of the mines.

If the iron ore of the mines of Elba was treated in this department, or in that of Var, the iron obtained might give rise to manufactories of this kind; but these considerations require further information, which will no doubt excite the attention of government.

*Department of Ardecbe.*

Coals are found (6) in several cantons of this department, and particularly in the environs of Jaujac, Privas, Aubenas, Vallon, and Saint Marcel d'Ardecbe. These mines, in general, are badly worked. They are interesting, however, in different points of view; first, on account of the consumption occasioned by the numerous manufactories of the country; and in the second place of the facility of carriage down the Rhone.

No data sufficiently exact have yet been obtained in regard to the products of these mines: they are certainly considerable, and would become much more productive and profitable, if the works were better conducted. The common price of coals in that country is about 8 cents per myriagramme.

An engineer of mines, if settled in this department, might introduce improvements in the working of these coal mines, which are exceedingly desirable. He could employ himself also on other objects useful to the industry of the inhabitants.

[To be continued.]

XIV. *On Canals.* By THOMAS TELFORD, Esq.\*

**I**NLAND navigation has now been so long and extensively practised in Great Britain, that the benefits arising from it are generally felt and acknowledged; and it is difficult to say, whether the improvements in agriculture, manufactures, or commerce, are most indebted to the numerous canals which now intersect the country in all directions, or to other causes.

It is not my intention at present to trace the rise and progress of this great national improvement, nor to enter deeply into the general principles of it: what I chiefly aim at in this slight sketch is, to draw the attention of the people who are concerned in laying out new canals, to some circumstances which appear to me to be very material to the interests of canal companies and to the country at large.

In considering the subject, I shall beg leave to state, that canals are chiefly for the purpose of,

1st, Carrying fuel and raw materials to some manufacturing towns or districts, and for exporting manufactured goods.

2dly, Carrying fuel for domestic purposes; manure for the purposes of agriculture; transporting the produce of the districts through which the canal passes to the different markets; and promoting agricultural purposes in general.

3dly, Carrying the produce of mines to the sea-shores.

These appear to be the leading points by which our views ought to be guided in planning new canals. The supplying the district through which the canal passes with groceries and merchant goods is a very secondary consideration; experience has taught this lesson to the persons who are concerned in the canals already made; and it is proper this source of disappointment should be made generally known, in order to prevent future misapplication of talents and capital.

In planning a new canal, it should be carefully considered under which of the foregoing heads the uses of the canal ought to be classed. Cases will sometimes, no doubt, occur, where the several purposes above mentioned will in some measure be combined; but they will rarely be so equally balanced, as to create a difficulty in discovering the predominant features. The general purposes of the canal being judiciously determined, all the following steps should be taken with a view to promote the principal end.

If the canal is for the purpose of carrying fuel and raw materials to some manufacturing town or district, and export-

\* Communicated by the Author.

ing the manufactured goods from thence, or for carrying the produce of mines to the sea-shores,—the line of canal should be made in as short a direction as the nature of the country will admit of, even if this line should be attended with some extraordinary expense; and that mode of conveyance should be adopted which is least liable to be interrupted, because in those cases much depends upon a constant and regular supply.

If the canal is chiefly for the purposes of agriculture, those lines are to be sought for which will accommodate the country most perfectly at the least possible expense. This will not, in general, be effected by short and direct, but, on the contrary, by circuitous and level lines, such as will visit many parts of the country, carry fuel and manure into every quarter, and take off the produce of the land for back carriage. Merchandize, although a very inferior consideration, will, by the same means, be extensively and equally distributed. When the carriage of those heavy but necessary articles is rendered cheap, and the supply convenient, the consumption will increase. The very use of lime as a manure is in general sufficient to create a considerable revenue upon a canal, when the coal and limestone can be carried along the canal at a moderate expense. As improvements in agriculture must increase the wealth of the inhabitants, more articles are required to supply their multiplied wants; the possession of capital prompts them to enter upon new employments, and the demands of all create an addition of business upon the canal.

Many other advantages attend circuitous and level canals. If there are upland countries above the level of the canal, some cheap and convenient situations may be selected where the waste water of the uplands can be collected into reservoirs to be formed for this purpose. From those reservoirs the water may be taken into the canals. At various points of the canal, where there are falls immediately on the lower side, mills may be erected for grinding corn, or for the purposes of manufactures: in many parts, two or three overshot wheels, of sufficient diameter, may be placed below each other: when the water has performed those useful offices, it will fall into the brooks, and prove a plentiful and regular supply to other works in the course of each stream.

Improvements in agriculture may also be greatly promoted by using a part of the waste water for the purpose of irrigation. For a canal carried over a country upon a high level would prove an extensive top drain. Not only the land adjoining the banks of the canal, but all which lies below the level, may enjoy the use of the water for irrigation; and the brooks and rivers will be the tail drains of the country.

Collecting

Collecting the waste water in winter, and in heavy rains, will prove advantageous in every respect: it will be the means of preventing high and rapid floods; it will be converting to useful purposes that which flows off to idle waste, and, not unfrequently, to positive mischief.

The useful purposes to which brooks and rivers are now applied, will not be injured, but improved; as there will be a more plentiful supply in summer, and a better regulated one during the winter months.

Parliament would not hesitate to grant canal companies the power of making reservoirs, and receiving a reasonable recompense for the water supplied from the canal for the purposes of agriculture and manufactures, because it would injure no person, and accommodate all: it will be furnishing additional powers to British industry, and creating permanent wealth to the nation.

I understand that this mode of managing water has long been practised in Italy, especially in the Milanese, where a considerable revenue is derived by the waste water of canals; and the regulations respecting it have long engaged the attention of the legislature of that country.

If I am right in the foregoing statements, it will be easy to apply them to different districts of country, and, by a careful and impartial judgment, to decide upon the character of each canal.

It is probably not sufficiently known what quantity of rain water falls in the course of a year in various parts of the kingdom; it therefore may not be improper to add the following statement, which will be some guide with regard to this subject; and if along with this we take the accounts of persons accustomed to make observations, a tolerably correct estimate may be formed of the average quantity of water which can be collected in each district: it will, in general, be found to exceed the expectations of persons who have not paid attention to this important subject.

Rain falls—At London, being the average of the following	
Years: 1774, 5, 6, 7, 8, 9, 1780; 1789, 90, 91, 92	Inch. 21 $\frac{1}{2}$
Upminster, in Essex, average of 1700, 1, 2, 3, 4, 5	19 $\frac{1}{2}$
Lincolnshire, in medium season	18
Ditto extreme wet	24
Liverpool	34 $\frac{1}{2}$
Townley, in Lancashire	42 $\frac{1}{2}$
Kendal, in Westmoreland	61 $\frac{1}{2}$
Dumfries, in Scotland	36 $\frac{1}{2}$
Glasgow, ditto	31

XV. *Proceedings of Learned Societies.*

## ROYAL SOCIETY OF LONDON.

**I**N the meeting of Jan. 27th was read a paper by Charles Gréville, esq. giving an account of three specimens, in different museums in France, of stones which have fallen to the earth from the clouds, all similar in their characteristics to those described by H. Howard, esq. and also of a mass of native iron which had fallen in Persia in the year of the Hegira 1030, according to the annals of the empire written by the emperor, and of which he is stated to have made some sabres and daggers: but until other iron was mixed with that of the mass described the iron was not malleable.

February 3. A paper by Everard Home, esq. on the nature of the tongue, was read. His experiments on different diseased tongues prove that that member is endowed with less irritability than any other organ of the body; and that a diseased part may be removed with great safety by means of ligatures.

On the 10th a letter from professor Bygge of Copenhagen was read, giving an account of the transit of Mercury in November last.

And on the same night was begun a paper by Dr. Herschel on the same subject, and also on the visible effects of sudden changes of temperature on the interior of telescopes; giving some observations on the proper situations to be chosen for observatories.

On the 17th the reading of this paper was concluded.

## ANTIQUARIAN SOCIETY.

It appears by a memoir presented to the Antiquarian Society on the 3d of February by Dr. Raine, that the inscription on the pillar near Alexandria, so long considered illegible, has been decyphered. The discovery was made by captain Leake of the artillery, and captain Squire of the engineers. The impression was taken in sulphur; and it seems that it was only when the sun shone on that side of the pillar where the inscription is, that any traces of it could be distinguished. By the inscription it appears that the pillar was erected in honour of the emperor Diocletian.

## VACCINE INSTITUTION.

Intelligence has been received at the Vaccine Institution through one of its members, that cow-pock inoculation has been introduced at Bombay, where it is in great estimation.

At the Vaccine Institution dinner on Monday Feb. 7th, many visitors voluntarily entered their names with liberal subscriptions. After dinner among other toasts were given, The promulgator of the vaccine inoculation, Dr. Jenner; Dr. Pearson, with due acknowledgments of his services in establishing the new inoculation by the evidence of practice.

Dr. Pearson then took the opportunity, after thanking the company for their honourable testimony, to inform them of many particulars concerning the conduct and objects of the institution, which could not be noticed in the report, and gave a most pleasing view of the advantages which individuals had already derived from the cow-pock: but he anticipated the momentous consequences of universal inoculation throughout the united kingdom in two respects.

1st, In preserving from 30,000 to 40,000 lives annually; and, 2dly, In a few years *exterminating the small-pox*. To accomplish these views, he contended that laws for the inoculation of every subject within a certain period after birth were essentially necessary, as well as immediate prohibition of the inoculation of the small-pox; that the latter measure alone would be quite inadequate; and he maintained that it was not more an infringement on the liberty of the subject, to render the cow-pock inoculation universal, than to prohibit, as already proposed, the small-pox inoculation. He observed that the number lost by inoculation for the small-pox was not an object of national importance, as it amounted to no more than six to nine per thousand; but when it was considered, that one could hardly say there was more danger from the inoculation of the cow-pock than from a puncture with a clean lancet, and that the symptoms were usually very much slighter than in the small-pox inoculation, and that by universal cow-pock inoculation, the infection of the small-pox must be extinguished, which end was obvious by even the universal inoculation of the small-pox; it remained only to institute the cow-pock inoculation as proposed, universally.

Dr. Pearson combated the objections against increasing the population, by showing that the resources of agriculture and animal food were not understood. That the augmentation of the population in France required, as a measure of safety, attention to the means of increasing this kind of national wealth;—that the mere waste in feeding of animals, and in preparing food for man, if taken care of, would be sufficient to support a considerably greater than the present population.

Dr. Pearson illustrated his argument in favour of vaccine

inoculation by observing, that going through the small-pox in the casual way, and by inoculation of it, and by exciting the cow-pock in place of the small-pox, was like crossing a river in three different ways.

1st, Swimming or wading over; in which 97 out of a thousand perished.

2d, Going over in boats; by which mode 6 to 9 per thousand were lost, and many others were injured in their health.

3d, By means of a bridge; during which passage no one perishes (except from those casualties which might happen in other situations), and by which generally the health was not affected, and when it was so it was as frequently amended as made worse.

#### ROYAL JENNERIAN SOCIETY.

It gives us pleasure to announce, that a new Vaccine Institution under the above title has just been established, which we have reason to hope, from the extensive patronage it has received, will eventually extirpate the small-pox, in paving the way to which so much had already been done by the Vaccine Institution.

His Majesty is Patron.

Her Majesty, Patroness.

*The Vice-Patrons are,*

His Royal Highness the Prince of Wales.

His Royal Highness the Duke of York.

His Royal Highness the Duke of Clarence.

His Royal Highness the Duke of Cumberland.

*Vice-Patronesses.*

Her Royal Highness the Princess of Wales.

Her Royal Highness the Duchess of York.

Her Royal Highness Princess Augusta Sophia.

Her Royal Highness Princess Elizabeth.

Her Royal Highness Princess Mary.

Her Royal Highness Princess Sophia.

Her Royal Highness Princess Amelia.

#### ROYAL SOCIETY OF GOTTINGEN.

*The Triple Inscription.*

In the sitting of September 4th 1802, professor Heyne read a memoir on the Egyptian monument with three inscriptions, of which the following is the substance:

The

The inscription contains a decree of the assembly of the priests of Memphis, by which they confer on king Ptolemy Epiphanes new divine honours, out of gratitude for the services rendered by him to religion, to its ministers, to the people, and to the state in general. To determine the time and occasion of this decree, is the first point in question, and therefore is the first object treated of in the memoir. Ptolemy Philopator left at his death (Olymp. 144. 1. 206 years before the birth of Christ) a son about five years of age. The most contemptible persons, who, under the preceding government, had ministered to the pleasures of Philopator, Agathocles, Agathoclea, and her mother CEnanthe, seized on the government and the guardianship of the young prince: they were followed by Tlepolemus, and afterwards by Aristomenes. In the year 200 before Christ the Egyptians intrusted to the Romans the tutelage of the young king, to protect the empire from the projects of king Philip, and of Antiochus of Syria. This measure produced internal troubles and revolts. When Thoas and the Ætolians had been crushed, it was thought most proper for the public repose to declare the prince, who was then fourteen years of age, major, and to suffer him to govern his affairs himself. The ceremony of the coronation, which is known under the name of *Anacleteria*, Olymp. 145. 4. 197 years before the birth of Jesus Christ, and nine years after the death of Philopator, was celebrated at Memphis. The ninth year is expressly named in the decree. All the Egyptian priests were invited to this ceremony. It appears that during the last years of the king's minority attempts had been made to gain over the people and the priests. Government had granted to the priests in particular a great number of privileges and immunities, and had also been at great expense for their worship and temples. Out of gratitude, the assembly of priests decreed, that in crowning the king (the day even is indicated in the text of the decree, the 4th of the Macedonian month Xantichus, the 18th of the Egyptian month Mechir, in the first half of our March, in the second half of the Olympic year) they should offer their homages to the king, according to the manner usual at that time, by giving him titles, and paying him honours, borrowed from the divine worship. The decree contains an account of all the services which the king had rendered to the empire and the Egyptian priests, and then the new titles of honour decreed to him.

The titles found in the decree are: the lord of kings, the glorious, the pious towards the gods, the conqueror of his enemies, the repairer of the pleasures of life, the lord of the

cycles of thirty years (Κυρίας των τριάκοντα ἐτηρων). He is styled also the descendant of the gods, (ἐκγόνος Θεων Φιλοπατορων) a title by which is understood his father Ptolemy Philopator, and perhaps his mother. The titles properly so called, which are repeated several times with the name, are: the enjoyer of long life, αἰωνοβίης, the favourite of Phtha, the visible god, ἐπιφανής, (and not illustrious, as is usually translated) the beneficent, ευχαρίστος, according to the meaning given to this word at that period.

The titles and names of the priests, which are at the head of the decree of the assembly, are: ἐφ' ἱερεως Αἰτοῦ, τῷ Αἰτοῦ, Αλεξανδρου καὶ Θεων Σωτηρων, καὶ Θεων Αδελφων, καὶ Θεων Ευεργετων, καὶ Θεων Φιλοπατορων, καὶ Θεου Επιφανοῦς ευχαριςτου. It is evident that these were priests of Alexander, of Ptolemy Soter, of Philadelphus Euergetes, of Philopator, of Epiphanes, and therefore of the royal family raised to the rank of deities. The name Αἰτος, eagle, gives reason to believe, that the case was the same here as with other kinds of priesthood mentioned in antiquity, that the family name was entirely abandoned, and could no longer be named; and that the rank was indicated by a general name. Here a symbolic name, viz. the eagle, seems to have indicated the high priest. A priestess of Berenice, no doubt the spouse of Soter, has the second rank; her name of honour is ἀλλοφορος. The third place, under the title of a *Canephora*, is given to the priestess of Arsinoë, the wife of Philadelphus. The fourth is occupied by the priestess of Arsinoë, the sister and wife of Philopator. Then comes ἀρχιερεῖς, προφηταί, καὶ οἱ εἰς το αδυτον εἰς πορευεμενοι, προς τον στολισμον των Θεων, (hence it appears that the statues were dressed or ornamented) καὶ πτεροφοραι καὶ ἱερογραμματεῖς, καὶ οἱ αλλοι ἱερεῖς, &c. respecting which Jablonski will give some explanations, as well as the learned who make further researches in regard to the explanation of the inscription. In regard to the word πτεροφοραι, one of the members remembered a passage in Clemens of Alexandria, where is found ἱερογραμματεὺς ἐφ' ὧν πτερὰ ἐπὶ τῆς κεφαλῆς\*. This class therefore was distinguished by a feather on the head, as a mark of honour, which is found also on the Egyptian monuments †.

An account of the benefits conferred by the prince on the priests affords several notices respecting the different kinds of taxes; but it would require too much room to detail the whole of them. They consist partly in pecuniary impositions,

\* See Beckmann's History of Inventions, part iv. p. 294.

† See *Adminanda Urbis Romæ*, No. 16. ed. 1693.

and partly in contributions in kind. We shall however mention those benefits which are connected with history. Among the services rendered by the king, is the siege and capture of Lycopolis. The insurgents of the last years had thrown themselves into this place, and had fortified it; so that the siege was attended with difficulties, especially in the *eighth* year (of the government). The Nile had increased in an extraordinary degree, and the king was obliged to oppose dykes to the water, in order that the siege might be continued with advantage. Polybius speaks of the siege of Lycopolis\*; but he places it in the 4th year of the 148th Olympiad, 185 years before the birth of Jesus Christ, and therefore 12 years later. Vaillant also places it in this year. The inscription evidently contradicts this opinion; but we need only examine with attention the words of Polybius, who speaks of the siege of Lycopolis as an event that took place some years before.

The honours decreed to the king consist partly in a confirmation of the antient honourable titles, and partly in the addition of a new one, that of protector (or avenger) of Egypt. Πτολεμαίου τῷ ἐπαμυνοντάς τῇ Αἰγυπτῷ, which was to be every where inserted in the sacerdotal rite, and then a statue was to be erected in all the temples. The statue was to be placed in such a manner, that the statue of the deity of the temple should present to it victorious arms. The priests were to adore the statue three times a day, in the sacred processions, when small golden temples (ναοί) were carried about, with small images of the gods. The image of the king was to be carried about in a similar sacellum.

Some other circumstances, which require further explanation, the author has reserved for another opportunity. Unfortunately the inscription has suffered in the lower part. It concludes with these words: (σ) τερεου λιθου τοις τε ιεροις και εγχωριοις και ελληνηοις γραμμασιν και στησαι εν εκαστωι των τε πρωτων και δευτερων (ιερων).

On the Persepolitan Inscriptions.

In the same sitting a memoir was read of M. George Grotefend, entitled: *Prævia de cuneatis quas vocant inscriptionibus Persepolitanis legendis et explicandis relatio*; the contents of which are the more astonishing, as the author is not an orientalist, and employed himself accidentally in decyphering this writing, hitherto so obscure. The author, who has long exercised himself in decyphering, in consequence of

\* Excerpt. lib. xxiii. 16.

a trifling dispute with one of his friends, laid a bet that he would decypher one of the Persepolitan inscriptions. He succeeded beyond his expectation, and in a few weeks was able to explain the greater part of the inscriptions, and to communicate details respecting the manner in which he proceeded, and concerning his results.

The author first speaks of the *cuneiform writing* in general, and lays down the following principles: 1st, The wedge-formed characters are really letters. On the monuments of Persepolis there are three sorts of them, which, for the most part, correspond with each other, as has been remarked by Niebuhr and Munter, and which may be called the first, second, and third kinds of writing. The whole three may be clearly observed on the vase of Caylus\*, provided two signs only be corrected from similar inscriptions found in Niebuhr and Le Bruyn. 2d. The wedge-formed figures are alphabetic letters, and not syllables or signs. In the first sort of letters the end of a word is indicated by a wedge placed obliquely, and in the second, by a perpendicular wedge. If the groupes of figures were syllables, we ought to have here words of ten syllables, for very often there are so many signs between two separations of words. In each of these kinds of writing there are distinguished about forty figures, a number which would be too small for writing in signs. 3d. All the wedge-formed inscriptions proceed from left to right, in a horizontal direction, and not vertically or in bustrophedon characters, as may be seen by comparing the inscriptions B, D, C, in Niebuhr. It is thence seen that this principle, expressed perhaps too generally, can be applied only to the inscriptions of Persepolis.

In the second section, which relates to inscriptions of the first sort in particular, the author observes, that this writing has need of marks proper for the long and short vowels, as in the antient Persian writing called the Zendic: hence the quantity of forty letters which Niebuhr has already collected. In a word, all the inscriptions which the author has been hitherto able to explain, relate to Darius the son of Hystaspes, and to Xerxes; a circumstance to which new researches have already given a high degree of probability.

In the third section the author describes the manner in which he proceeded, and gives his explanations. The suppositions already mentioned, as well as the analogy of the inscription of the Sassanides, gave reason to expect in particular the name of king, and titles particularly for Darius and

\* Rem. d'Antiq. v. pl. 30.

Xerxes. The words on which accents are found ought to be titles. By different combinations the author obtained the words *Kbscherche* and *Darbeusch*; by help of the words found, read the other words of the inscription on the urn of Caylus, and those of B and G of Niebuhr, which he explains according to the Zendic. The dictionaries and grammatical remarks of Anquetil were of great service to him in this labour.

The author has given, as the results of his researches, an explanation of the inscriptions on the urn already mentioned, and those of Niebuhr, pl. 24, B, G. He read thus the B of Niebuhr: *Dárbeusch Kbschbiób egbre. Kbschbiób Kbschébiobetcháo. Kbschébiób. Dabutchao. Goschlasphæbe, bún akbeotchoschoch, áb ôoo, Môro, ezutchusch*, that is to say, *Darius rex fortis, rex regum, rex daharum (filius) Hystaspys, stirps mundi rectoris, in constellatione masculá τοῦ Moro, τοῦ Ixed.* Moro is one of the twenty-eight constellations; the author refers the word governor of the world to Giemshid, from whom the Persian kings, the Achemenides, derived their origin.

He read thus the inscription marked G of Niebuhr: *Kbscherche Kbschébiób egbre. Kbschbiób. Kbschébiobetcháo. Darbeusch Kbschbiobêbê. bún. akbeotchoschób*, that is to say, *Xerxes rex fortis, rex regum (filius) Darii, regis stirps omnium rectoris.* It is in this manner that the author explains the inscription found in Le Bruyn, p. 273, No. 133, on the mantle of the king: he even proposes corrections. He thinks also that he can restore and correct the inscription on the window, (see in the same place No. 134.) What has been here said may enable the reader to form some opinion of his labour.

However unexpected this discovery may be, it seems to deserve some attention: we have reason indeed to be on our guard against discoveries of this kind, because one may be easily deceived by possible combinations, and by certain suppositions which may originate in chance; especially when the language affords no certain means of judging of their correctness, and when, on the other hand, the language itself must in some measure be discovered. As the author has not made known his alphabet, and as he has not mentioned whether it be complete for all the inscriptions of the first sort of writing, and how far he has made researches in regard to the other kinds; no opinion can yet be formed of the certainty and extent of the discovery. It appears that the author hitherto has employed himself chiefly with the inscriptions of Persépolis, and we must confine to these inscriptions some principles too generally expressed; namely, that each sort of

writing exhibits forty signs, that all the wedge-formed writing has a horizontal direction, and that they are all alphabetic. There is something also to be rectified in the explanation: *rex dabarum*, for example, as a title of Darius, is not very probable: in regard to *rektor mundi*, it might be referred perhaps with more reason to Ormuzd. But what may give us a prepossession in favour of the author's hypothesis is, that it is founded on what the researches and observations hitherto made, give as the most probable result. From these indeed we can admit that the buildings of Persepolis belong to the time of the successors of Cyrus. We may even go further, and conclude that the greater part of the monuments were finished under Darius and the following kings; because the short duration of the reign of Cambyzes would not have been sufficient for the execution of such works.

As the inscriptions are as old as the monuments, they must be referred to the time of these princes. In a word, it is seen that the wedge-formed writing on those monuments, and in general where it has a horizontal direction, proceeds from left to right. This circumstance, which was observed by Niebuhr, is incontestably proved by a similar inscription on stone found in the French national museum at Paris, and which C. Millin has given in the first number of *Monumens Antiques*, plates 8 and 9. The artist in this inscription has placed insulated wedges, or groupes, or whole letters, for which there was no room, above the lines, or in the second column, and always to the right, where the end of the line ought consequently to be. The author's explanation agrees with these observations. He reads from left to right, and finds in the inscriptions allusions to Darius, Xerxes, and certain points respecting the worship of the magi. The legends which the author has found are consistent and probable, when considered historically. They have also the greatest analogy with the inscriptions of the Sassanides, a dynasty descended from the antient Persian kings, and who endeavoured to re-establish the empire and its religion. As these princes on antient coins and monuments are styled kings of the kings of Izan, of divine origin, &c. similar titles, but more simple, are found in these inscriptions. Time alone can show whether the continuation of the author's researches will justify these explanations.

If the learned should succeed in decyphering the wedge-formed writing, their discoveries would throw great light on many points of the Asiatic antiquities, because more monuments of this writing are daily discovered. They may be divided into three classes. 1st, The Babylonian, among which are reckoned the bricks of the walls of antient Babylon,

lon, and perhaps some other monuments, if the researches of Dr. Lichtenstein do not assign to them another period. This writing has a peculiar character, which may be distinguished by the name of the *nail-writing*\*. 2d, The Persian: the monuments of Persepolis, some gems and vases, the stone of which C. Millin speaks, and several others. The writing of the latter has more resemblance to the point of a dart. It might be called *dart-writing*. 3d, The Egypto-Persian: to this kind, in all probability, belong the cylindric amulets of hæmatites with wedge-formed letters and figures; and, in particular, a fragment of a stone found near Suez, which exhibits wedge-formed letters; and a Persian head, having on it a hawk's wing. An engraving of it may be seen in Denon, from a drawing by general Dugua. This variety of monuments, which is daily becoming more numerous, proves how widely diffused the use of such letters was at a certain period.

## XVI. *Intelligence and Miscellaneous Articles.*

### ANTIQUITIES.

SOME curious and valuable remains of antiquity were lately discovered in Istria, Dalmatia, and Albania, and have been sent to Vienna by M. de Carnea Stephaneo, his imperial majesty's commissary in these provinces. The articles found are:

1st, A torso of Parian marble, found among the ruins of Salona, in Dalmatia, a league north-east from Spalatro. It is seven feet and a half in height, and represents a naked warrior seated on the trunk of a tree, on which is placed his war dress. This statue is in the most sublime Grecian style. The expression and beauty of the form show that it is the work of one of the first masters of antiquity. The belt exhibits a peculiarity not generally met with in statues of this kind. It is much to be regretted that it is mutilated nearly in the same manner as the torso of the Vatican at Rome.

2d, A Minerva, of Corinthian brass, found in Dalmatia, near Xa-Ostros, three leagues and a half from the mouth of the Narenta. It is eight feet in height, and perfectly entire. This Minerva is in the Greek costume, with a helmet, an ægis, and a Medusa's head on the breast: she holds a lance in the left hand, which is somewhat elevated, and a cup in the right. The hair is arranged in the same manner

\* For a specimen of this kind of writing, see our xith volume.

as is still usual among the Illyrian women in the interior part of Dalmatia, and among those of Kamtschatka and on the banks of the Anadir.

3d, A Madona with infant Jesus asleep, painted by Aloys Vivarinus, of Murano, on a plaster ground. This painting, which was finished in 1489, is five feet two inches in height and one foot eleven inches in breadth. It was executed at the time when the art of painting in oil was first carried from Flanders to Italy. It proves that Vivarinus, by his delicate and expressive pencil, had already carried to a high degree of excellence the art of painting, which was not brought to a state of perfection till the time of Charles V.

4th, A naval battle painted in oil, eleven feet in height and twenty-one in breadth. It is a master-piece of Tintoretto. He composed it at the request of the senate of Venice, who presented it to the city of Pisano. It represents the battle said to have taken place in 1177 between the Germans and the Venetian fleet near Cape de Salvora, in Istria, and which was first mentioned by Andrew Dandolo, in his Chronicle, two centuries after.

5th, A bust, painted on marble, of Sanctorius, a celebrated professor of Padua, who died in 1639, and who formed an epoch in the philosophical history of medicine.

#### EARTHQUAKE.

Some observations have lately been published in the German journals in regard to the curious circumstances which accompanied the earthquake that took place in the province of the Seven Mountains towards the end of October. It was in the low meadows lying towards the north that the phenomena were most sensible, where deep fissures two or three feet in breadth and thirty-five fathoms in length, which divided into several branches, were formed in a direction from west to east. A great deal of very fine sand of an ash-gray colour, which emitted a sulphureous odour, was thrown up from these fissures. The principal fissure, after extending throughout a long space, separated into several branches, which bent themselves into semicircles towards the west, and traversed a house and a stable, after piercing the walls at the foundation. The whole country was covered with one or more inches of sand in the direction of the fissures: and persons who were witnesses to these phenomena assert, that at the time of the last shock the earth opened with a noise like that of a musket shot; that a black and strong smelling vapour issued from the fissures, as well as a great deal of water, which spouted up with a loud noise; and that these jets were followed by a great quantity

quantity of sulphureous sand. The ground also sunk down in a sensible manner. The water which issued from the fissures was exceedingly sulphureous, cold, and limpid.

Chemists observed, that a pound of this sand diluted in eight pints of water produced no effervescence, and even gave no precipitate on being mixed with vitriolic acid. The nitrous acid gave the same result. It was only after its decomposition, and after gall-nuts were able to produce no change in the colour, that the water became turbid, in such a manner as to show the presence of the vitriolic acid. By decoction and evaporation signs of crystallization were observed at the surface; and, when the evaporation was complete, there remained about eight grains of alkaline matters, two parts of which were Glauber's salt, one earthy salt, and the other gypsum.

TRANSIT OF MERCURY,

*As observed at Paris November 9, 1802.*

Observers.			Interior Contact.			Exterior Contact.		
C. Mechain	-	-	0 <sup>h</sup>	6 <sup>m</sup>	45.4 <sup>s</sup>	0 <sup>h</sup>	8 <sup>m</sup>	30.4 <sup>s</sup>
Lalande	-	-	0	6	29.0	0	7	56
His nephew and	-	}	0	6	43.0	{	8	18
Burckhardt	-		0	6	49		8	19
Messier	-	-	0	6	49.2			

The above results are mean time reduced to the national observatory.

Mechain saw the planet very well terminated without any aureola, though he always followed its progress on the sun with a telescope of three object glasses  $4\frac{1}{2}$  inches in diameter and 7 feet focus, with which he took 45 distances. He had observed in the same manner the transits in the years 1782, 1786, 1789, and 1799.

CHEMISTRY.

1. Brugnattelli has lately observed, that by treating paper with the nitric acid, a large quantity of suberic acid mixed with oxalic acid is obtained. This seems to prove that Fourcroy was right in classing cork among the immediate principles of the vegetable kingdom.

2. The same chemist in a letter says, that after a particular examination of wheat, he thinks he has found that vegetable gluten is nothing else than animal fibrous matter. It differs indeed from animal gluten or glue, by its insolubility in cold water, and by the contraction it experiences in warm water. When treated with the nitric acid it swells, produces

duces a foamy effervescence, gives out nitrous gas, and dissolves. This solution is decomposed by water, like that of animal fibrous matter, and the white precipitate thence resulting dissolves in ammonia, assuming a dark yellow colour.

3. A discovery of two new principles in sulphureous waters has lately been made by Counsellor Westrumb, of Hammeln, who gives the following details:

“For six years I have been exclusively employed in the analysis of sulphureous waters. I have found an easy and entirely new method of determining what they contain of sulphurated hydrogen and carbonic acid gas; and I have discovered a principle hitherto unknown in these waters; namely, bitumen in combination with sulphurated hydrogen gas and hydro-sulphurated lime. I communicated the detail of this discovery to M. Wurzer. Counsellor Von Crell and Professor Schaub of Cassel saw at my house these new products. These principles are found in the waters of Neuwdorf, Limmen, Rehburg, and Eylse. In the last place, five sulphureous springs, altogether different from each other, a spring of carbonic gaseous water, and one of fresh water, are found in an extent of less than a thousand square feet. Near Buckembourg, the capital of the small country of Leppe, to which Eylse belongs, are found two other sulphureous springs perfectly similar, one of which we sounded to the depth of eighty feet without finding its reservoir. Baths of mud similar to those of St. Amand have been constructed at Eylse: a very considerable quantity of hydro-sulphurated mud, which may furnish baths for several centuries, has been found there.”

4. A new gas, sulphurated azotic gas, has been discovered in the mineral waters of Aix-la-Chapelle by M. C. Gimbernat, sub-director of the museum of natural history at Madrid, who gives the following account of it:

“I have just finished my labour on the analysis of the sulphureous waters of Aix-la-Chapelle. I have found that the excipient of the sulphur in these waters is not hydrogen, as has hitherto been supposed and asserted, but azote. These waters contain no sulphurated hydrogen.

“Sulphurated azotic gas is a chemical body, the existence of which has never been suspected. Nature presents it in abundance in the mineral waters of this country.

“Besides sulphurated azote, these waters contain a great deal of pure and free azote interposed, and in the semi-gaseous state.”

5. We

5. We are informed by Mr. Paul of Gèneva (now in London) that water holding sulphate of lime in solution, and impregnated under pressure with hydrogen gas, of which it can take up about one-fourth of its volume, after standing stopped up for about three months, on being opened was found to contain sulphurized hydrogen gas. This has been repeated several times with the same result. In distilled water not impregnated with sulphate of lime, the hydrogen gas undergoes no change.

How is the sulphurized hydrogen gas produced? There is plainly a decomposition effected, not merely of the sulphate of lime, but of the sulphuric acid, which parts with a portion of its sulphur.—What becomes of the oxygen? Does it join the hydrogen, and form water?

The hydrogen gas made use of in the above experiments was procured from the decomposition of water, by applying it to iron exposed to a high temperature. When obtained by means of sulphuric acid, water, and iron, in the humid way, it has a different flavour, somewhat hepatic.

6. Mr. Paul has mentioned to us another circumstance, which deserves to be made known. Water impregnated with hydrogen gas is extremely injurious in dropical complaints. A gentleman labouring under the dropfy was cured by drinking water impregnated with oxygen gas; but during the cure his servant by mistake had purchased some hydrogen water. In less than 24 hours his malady returned: on finding out the cause, and having recourse to the oxygenated water, the mischief was speedily remedied.

#### ANIMAL GALVANISM.

Some curious Galvanic experiments were made on February 18, by professor Aldini, in Dr. Pearson's lecture-rooms. They were by far more interesting and satisfactory than any we have yet noticed on animals, owing to the pains taken to procure the fittest subjects for the operations. They were instituted in the presence of his excellency the ambassador of France general Andreossi, lord Pelham, lord Roxburgh, lord Castlereagh, lord Hervey, the honourable Mr. Upton, Mr. Cholmondely, Mr. Anchora, Mr. Elliot, and several other gentlemen of rank. The professor was assisted ably, as on former occasions, by Mr. Carpue, Mr. Cuthbertson, and Mr. Hutchins.

Among other important facts, it was decisively shown,

1. That a vital attraction subsists between a nerve and muscle: for the suspended sciatic nerves of a frog, after detaching

taching the spine, being brought near the intercostal muscles of a dog, while the assistant who held the frog did, with his other hand, touch the muscles of the thigh of the dog, (thus forming a circle); in this situation, the nerves suspended approached, and came into contact with the muscle, as evidently as a silken thread is attracted by sealing-wax.

2. The heart of a rabbit was excited to action in a little time after the animal was killed; but vitality disappeared much sooner than in the other muscles: so that this organ is the *primum*, and not, as Harvey asserted, the *ultimum moriens*. The lungs, liver, and spleen could not be excited to action, even immediately after the animal was killed.

3. The most important fact of all was that of exciting contractions by making a circle of nerves and muscles of different animals, without any metallic exciter or conductors.

4. The head of an ox, recently decapitated, exhibited astonishing effects: for, the tongue being drawn out by a hook fixed into it, on applying the exciters, in spite of the strength of the assistant the tongue was retracted, so as to detach itself, by tearing itself, from the hook: at the same time a loud noise issued from the mouth by the absorption of air, attended by violent contortions of the whole head and eyes.

After the exhibition of these phænomena of Galvanism, which, among other philosophers, particularly occupy the attention of the First Consul, the company partook of an elegant *dejeune* and a *conversazione* in Dr. Pearson's house.

#### XVII. Description of Mr. PEPYS's large Galvanic Apparatus.

MR. PEPYS jun. has lately constructed the most powerful Galvanic apparatus that has, we believe, been yet produced, of which we shall endeavour to give our readers some idea. It consists of 60 pairs of zinc and copper plates disposed in two troughs constructed on Mr. Cruickshank's plan, but with some accompanying arrangements which are extremely convenient and useful. That our description may be the better understood, we refer to a view of the apparatus, Plate I.

A A. The two troughs, each consisting of 30 pairs of plates. The plates are cemented into troughs made of mahogany, properly varnished to stand the action of the fluids which may be introduced into the cells between the plates. The plates are each 6 inches square, or contain 36 square inches of surface on each side; and that they may be able to conti-

nue in use for a considerable time even with the application of strong acidulous liquors, they are made of such a thickness that each pair weighs four pounds. The troughs are furnished with pivots at each end, by which they rest on the bar B of the table into which the whole apparatus is framed: on these pivots, which are so placed as to throw the centre of gravity of the troughs below them, the troughs revolve when it is necessary to empty the cells.

C. A japanned iron tray or receiver, capable of holding the contents of the cells of the troughs when they are emptied.

D. A range of six funnels made of tin, the distances of the tubes of which are so adjusted that they enter six of the cells of the troughs at once.

E. A tin vessel having six distinct cells with spouts, each capable of containing the exact quantity of fluid necessary to fill a cell of the trough.

By means of the funnel D and vessel E, the troughs are filled with very little trouble, no more being necessary than to fill the vessel E by dipping it into the fluid or acid, and then emptying it, by the spouts corresponding with the divisions in the funnel D, into the latter, the pipes of which are placed in the corresponding cells of the troughs.

FF the two prime conductors, being two brass rods with shoulders. They pass through holes in the top of the table (which, to show the arrangement of the troughs, appears elevated in the engraving) into the end cell of each trough.

GG two metallic conductors, each consisting of two parts joined by sockets, and each having a socket which fits upon the prime conductors FF. By this contrivance they are movable in any direction required in the experiments.

HH two shallow vessels made of tin, with tubes underneath, by means of which they can be placed on the tops of the conductors. When filled with water they are found convenient for giving the Galvanic shock in a very perfect manner, and for other experiments.

I, an arch of metal to unite the troughs at their further ends.

When the cells are filled, the troughs connected by the metallic arch, the top or lid shut down, and the prime conductors put in their places, the apparatus forms a Galvanic table free from any incumbrance, and extremely well adapted for every experiment in Galvanism.

On the 21st of February, with a number of scientific gentlemen, we witnessed a trial of this apparatus. The experiments

ments made by Mr. Pepys on the deflagration of metals were the most brilliant and splendid we ever beheld.

The troughs were filled with 32 pounds of water mixed with two pounds of concentrated nitrous acid. With this charge—

Iron wires of  $\frac{1}{16}$  to  $\frac{1}{8}$  of an inch in diameter were deflagrated with great splendour. A number of the small ones twisted together produced somewhat like a little brush: a pleasing appearance in deflagration.

Charcoal of box wood was not only deflagrated at the place of contact, but remained permanently red hot for near two inches in length.

Lead foil burnt with great vividness, becoming red hot, and emitting a small volcano or adjutage of red sparks along with the flame.

Tin foil burnt with great splendour, with smoke and sparks.

Dutch leaf or brass foil deflagrated vividly, with smoke and a profusion of sparks.

Silver leaf burnt with an intense vivid green light: no sparks, but much smoke or fume.

Gold leaf deflagrated with bright white light and smoke.

Tin wire,  $\frac{1}{8}$  of an inch in diameter, fused, burnt, and oxidated, with great splendour.

Platina wire,  $\frac{1}{16}$  of an inch in diameter, became red hot, white, and fused into globules at the contact.

Gunpowder, phosphorus, and inflammable substances, are instantly fired by contact with conductors armed with charcoal.

The Galvanic power was capable of deflagrating charcoal, after passing through sixteen persons with wetted hands joined.

The best method of showing these experiments is by introducing into an earthen plate filled with clean mercury, one of the prime conductors. The other conductor may be then mounted with the several substances to be tried. The leaves and laminæ of metal easily adhere to it by wetting. Wires may be twisted round it, and will also serve to bind other substances to it.

The power of this large trough was so great as to keep up the deflagrations and combustion without intermission.

XVIII. *An Essay on the Method of determining the Difference of Longitude between Places at Land, from the observed Transits of the Moon over their Meridians; with a Demonstration and Example.* By Mr. GAVIN LOWE\*.

Ea est methodorum simplicissimarum ratio atque natura, ut postremæ in mentem veniant, et nisi obstinatior animo, ne veniant quidem.

BOSCOVICH.

**ALTHOUGH** the method of determining the difference of longitude at sea from the lunar observations has been accurately laid down by Dr. Maskelyne and other able nautical astronomers, it has, however, happened that several writers on longitude and astronomy have, in the course of the last twenty years, given rules for finding the difference of longitude at land from the moon's transits, either so erroneous or imperfect, that the adoption thereof might do a serious injury both to navigation and geography: they have given examples, but no demonstrations; or at least such obscure and imperfect ones, as prove that they had not a clear conception of the matter.

It is for these reasons that the following demonstration of a rule both easy and accurate for finding the difference of longitude, is now proposed. The data are the observed increase of the moon's right ascension in passing from the first to the second meridian, and the increase of the sun's and moon's right ascension in twelve hours apparent time, which may be had from the Nautical Almanac.

DEMONSTRATION.

Let the circle  $ABC$  represent the equator,  $P$  its pole, and  $APD$  the first meridian, as that of Greenwich. Suppose that the centres of the sun, the moon, and a fixed star are on that meridian at the same moment of time as represented at  $A$ , and that they move from thence to the westward with their respective velocities, the earth being considered as at rest. Then, after twelve hours apparent time, the sun will be at  $D$ , the opposite point to  $A$ , or  $180^\circ$  distant from it; but the fixed star, moving in appearance over a greater space than  $180^\circ$  in twelve hours apparent time, will be at  $E$ ; while the moon, with a motion apparently slower than the sun and the star, will appear after twelve hours at the point  $B$ , or on a meridian  $BP$ . But  $ED$  is the distance of the sun from the star

\* Communicated by the Author.

after an interval of twelve hours apparent time, and  $EB$  the distance of the moon, or, in other words, the increase of their respective right ascensions: and since  $ED$  and  $EB$  are known from the Nautical Almanac, if we subtract the first from the last, we have  $DB$ , equal to the difference between the increase of the sun's and moon's right ascension in twelve hours apparent time. Now the difference of longitude between the two meridians  $AP$  and  $BP$  is the arc  $A\beta B$ , equal to  $A\beta D$  less the arc  $DB$ ; that is, equal to  $180^\circ$  less the difference between the increase of the sun's and moon's right ascension in twelve hours: and, since the increase of the moon's right ascension from the time of its passing the meridian  $AP$  to the time of its passing  $BP$  is known from observation, and equal to  $EB$ , we can make the following proportion for finding the difference of longitude between any other two meridians,  $AP$  and  $\beta P$ , from the observed increase of the moon's right ascension  $\varepsilon\beta$ .

As  $EB : A\beta D - DB :: \varepsilon\beta : A\beta$  the difference of longitude; or, in more familiar language, as the increase of the moon's right ascension in twelve hours apparent time is to  $180^\circ$  or  $12^h$  less the difference between the increase of the sun's and moon's right ascension in that time ::, so is any other observed increase of the moon's right ascension between two meridians: to their difference of longitude. Q. E. D.\*

If the increase of the moon's right ascension in twelve hours were uniform, or such that equal parts of it would be produced in equal times, the above rule would be strictly accurate; but as that increase arises from a motion continually accelerated or retarded, and seldom uniform but for a short space of time, it will therefore be necessary to find the mean increase of the moon's right ascension when it is at the intermediate point between  $A$  and  $\beta$ , in order to determine their difference of longitude with the greatest precision; and for that purpose, Taylor's Tables of Second Difference are very useful.

#### EXAMPLE.

April the 8th, 1800, the transit of the moon's first limb was observed at the royal observatory ( $A$ ); and, allowance being made for the error of the clock, its right ascension was

	12 <sup>h</sup> 35 <sup>m</sup> 18.22 <sup>s</sup>
Add the time that the moon's semi-diameter took to pass the meridian	0 1 8.38

	<div style="border-top: 1px solid black; display: inline-block; width: 100%;"></div> 12 36 26.6
--	---

Right ascension of the moon's centre

12 36 26.6

\* Demonstrated for the first time by the author in 1770.

Brought over	12 <sup>h</sup>	36 <sup>m</sup>	26.6 <sup>s</sup>
On a meridian ( $\beta^*$ ) far to the westward the transit of the moon's first limb was observed the same day, and being reduced to the centre, its right ascension was	-	12	47 56.7

Increase of right ascension between A and $\beta$	-	-	-	0	11	30.1
---	---	---	---	---	----	------

The increase of the moon's right ascension in twelve hours apparent time per the Nautical Almanac was	-	-	-	0	26	3†
---	---	---	---	---	----	----

The increase of the sun's in the same time	-	-	-	0	1	49.65
--	---	---	---	---	---	-------

Difference	-	-	-	0	24	13.35
------------	---	---	---	---	----	-------

And 12 hours minus this difference is = 11<sup>h</sup> 35<sup>m</sup> 46.65<sup>s</sup>; therefore, As 26<sup>m</sup> 3<sup>s</sup> : 11<sup>h</sup> 35<sup>m</sup> 46.65<sup>s</sup> :: 11<sup>m</sup> 30.1<sup>s</sup> to 5<sup>h</sup> 7<sup>m</sup> 12<sup>s</sup>, the correct difference of longitude between A and  $\beta$ .

By reducing the three terms to seconds, and using logarithms, the operation is much shortened.

In a book published by Mr. Mackay on longitude about fifteen or sixteen years ago, there is a rule given, and also an example, for finding the difference of longitude at land from the transits of the moon, but no demonstration. The rule, when divested of its high-sounding enunciation, runs thus:

As the increase of the moon's right ascension in twelve hours apparent time : is to 180° ::, so is any other observed increase between two meridians : to their difference of longitude. It follows from this, that the moon as well as the sun would, in twelve hours apparent time, pass over an arc of 180°, although the apparent motion of the moon to the westward in twelve hours, or 180° of space, be less than that of the sun by six or seven degrees; and so much error would this method produce, if the two places differed about 180° in longitude.

The above example, wrought according to Mackay's rule, would come out thus:

As 26 <sup>m</sup> 3 <sup>s</sup> : 12 <sup>h</sup> :: 11 <sup>m</sup> 30.1 <sup>s</sup> to	-	5 <sup>h</sup>	17 <sup>m</sup>	53.7 <sup>s</sup>
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But the correct difference as above is	-	5	7	12
--	---	---	---	----

Error	-	-	0	10	41.7
-------	---	---	---	----	------

which amounts to more than  $2\frac{1}{2}^\circ$ , or 150 miles, in a difference of longitude little exceeding five hours.

\*. A place near Port-Royal, in Jamaica.

† By interpolation 26<sup>m</sup> 3.67<sup>s</sup>; but the fraction of the second was not minded.

Mr. Edward Pigott adopts the very same rule for determining the difference of longitude between Greenwich and York, and states the result in the *Philosophical Transactions* for 1786, p. 417.

Mr. Vince has inserted this rule and example in his *Treatise of Practical Astronomy*; but we have to regret that they were not accompanied with a strict demonstration.

The Rev. Mr. Wollaston, in the appendix to his *Fasciculus Astronomicus*, published two or three years ago, has given a rule, without demonstration or example, for finding the difference of longitude from the moon's transits, which produces the same error as Mackay's and Pigott's, although worded differently from theirs. Mr. Wollaston makes the first term of his proportion apparent, and the third mean time; this renders the result erroneous. Since the motion of the sun, moon, and planets are computed for apparent time, and given so in the *Nautical Almanac*, mean time is not at all requisite for resolving the difference of longitude either at sea or at land. We shall therefore endeavour to apply Mr. Wollaston's rule, according to its literal meaning, for finding the difference of longitude from the above observations.

The right ascension of the moon's centre on the meridian of Greenwich being known, we can easily deduce the mean and apparent time corresponding to it; and in like manner the mean and apparent time at the distant meridian  $\beta^*$ . The apparent and mean time of the transits of the moon's centre over the meridians of A and  $\beta$ , when strictly computed, were as follows:

		Apparent Time.	Mean Time.
At A	- -	11 <sup>h</sup> 26 <sup>m</sup> 47.81 <sup>s</sup>	11 <sup>h</sup> 28 <sup>m</sup> 33.5 <sup>s</sup>
At $\beta$	- -	11 37 29.5	11 39 11.4
Time later at $\beta$ than at A		0 10 41.69	0 10 37.9

From the increase of the moon's right ascension in twelve hours - -

26 3

Subtract the increase of the sun's right ascension in that time - -

1 49.65

The moon's retardation in twelve hours - -

24 13.35

\* Mean time, however, at  $\beta$ , before its longitude be known, is not a fair postulatam. The method above demonstrated does not require it.

Then, "As twice the moon's retardation in twelve hours : is to twenty-four hours ::

"So is the mean time later at  $\beta$  than at A : to the difference of longitude west from A."

After doubling  $24^m 13.35^s$ , and also 12, which is totally unnecessary, as the result would be the same if they stood single, we state the following proportion :

As  $48^m 26.7^s : 24^h :: 10^m 37.9^s$  to  $-5^h 15^m 1.3^s$ , the difference of longitude between A and  $\beta$ .

But as the third term is improperly reduced to mean time, we shall take the apparent time above found, and then  $48^m 26.7^s : 24^h :: 10^m 41.69^s$  to  $-5^h 17^m 53.7^s$ ; the same as results from Mackay's and Pigott's rules.

We shall only remark, that  $5^h 17^m 53.7^s$  is the apparent time that the moon took in passing from the meridian of A to the meridian of  $\beta$ ; but from what has been demonstrated, the apparent time at  $\beta$  will be equal to the difference between the increase of the sun's and moon's right ascension in that interval of apparent time; for DB, or  $24^m 13.35^s$  is the difference for twelve hours, and therefore by proportion  $\delta\beta$ , or  $10^m 41.69^s$  will be the difference for  $5^h 17^m 53.7^s$ ; subtracting the former from the latter, we have  $5^h 7^m 12^s$ , the difference of longitude as before, and a clear proof that the authors above mentioned have omitted to deduct the apparent time at the distant place or station  $\beta$ , from the apparent time at Greenwich.

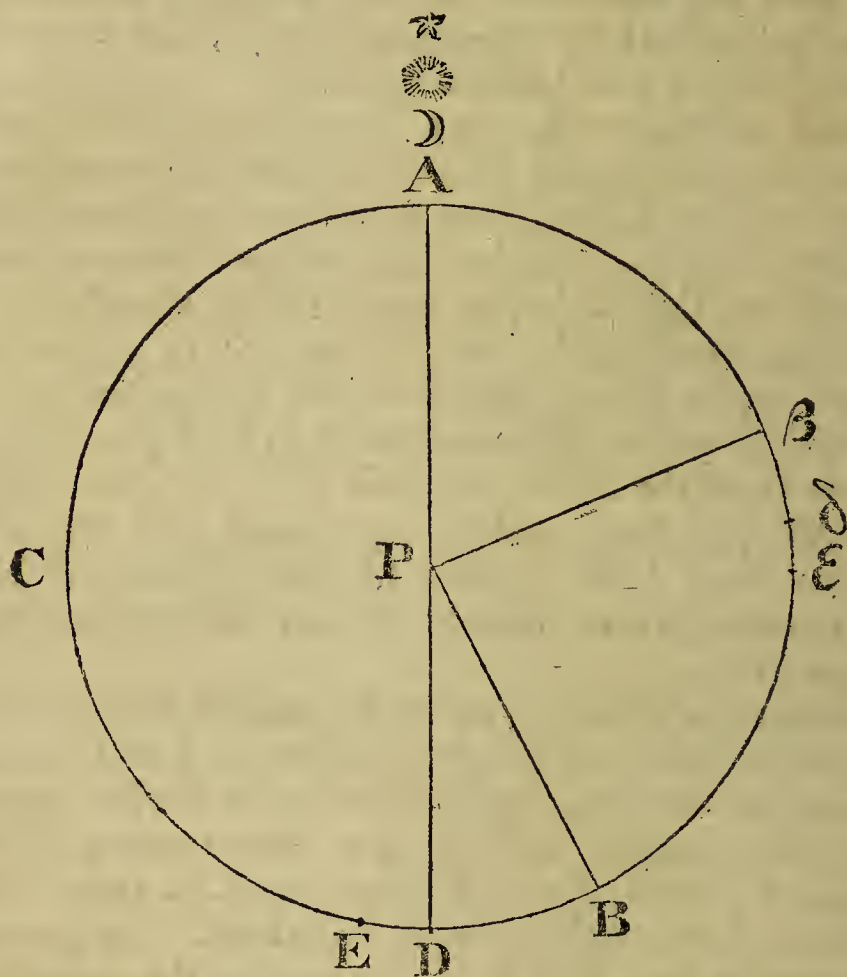
If it should be thought easier to employ sidereal time in resolving the difference of longitude between A and  $\beta$ , let ED, the increase of the sun's right ascension in twelve hours, be added to twelve hours, and we have then the arc of sidereal time ABDE; from which subtracting EB, there remains A $\beta$ B, equal to the difference of longitude. By proportion we can therefore say, As EB, the increase of the moon's right ascension in twelve hours; is to ABD + DE, or twelve hours + the increase of the sun's right ascension; so is any other observed increase of the moon's right ascension as  $\epsilon\beta$ ; to the arc of sidereal time A $\beta\epsilon$ ; and if from this we subtract  $\epsilon\beta$ , then A $\beta$  is the difference of longitude required.

To apply this rule for finding the difference of longitude from the above observations and data, we say,

As  $26^m 3^s ; 12^h 1^m 49.65^s :: 11^m 30.1^s$  to A $\beta\epsilon$   $5^h 18^m 42.1^s$   
 From this subtract  $\epsilon\beta$ , the observed increase  $0 \ 11 \ 30.1$

The difference of longitude between A and  $\beta$  is  $5 \ 7 \ 12$   
 Q. E. I.

Although we can safely trust to the accuracy of the observations at the royal observatory, yet, at the distant station  $\beta$ , great attention must be paid in adjusting the transit instrument truly to the meridian, and taking care that it be perfect in collimation before the moon's transit be observed there; and also, that correct time be deduced as soon before or after the moon's transit as possible, from observing the meridian passage of one or more of the thirty-six stars, whose right ascensions, after a long series of observations, have been ascertained by the Astronomer Royal with the greatest precision.



In order to put the above method to the test of experiment, a number of transits of the moon's first limb were observed many years ago at a place 26 seconds of time west from the royal observatory, with a small portable transit instrument made by that eminent artist Mr. Edward Troughton, of Fleetstreet. It had a telescope 20 inches long, a treble object-glass, and the price only 20 guineas. When the observations at Greenwich were published, all those that were made on the same days at both places were employed to determine the difference of longitude, and the mean result of four or five corresponding observations gave it true to two seconds of time.

This is only mentioned to show, that with small transit instruments well constructed, and due attention paid to their adjust-

adjustments, observations may be made sufficiently accurate for answering the ends of practical astronomy.

We expect soon to have the lunar tables brought to a greater degree of perfection, by applying certain equations that have been lately discovered, and by which those purposes will, of course, be more successfully promoted. It is also much to be wished that the moon's transits were as assiduously observed by public and private astronomers in every country as they are at the royal observatory in this, and from time to time communicated to the public at large.

It sometimes, but not very frequently, happens, that favourable weather will permit one astronomer to observe four transits of the moon over his meridian in the same number of succeeding days. If another, or indeed a hundred others, should observe only one or two on any of those days, whether at the nearest or remotest stations from the first meridian; such observations are of great value, and afford the very best means of finding the difference of longitude with accuracy, because the errors of the lunar tables are thereby completely obviated.

Paradise Row, Islington,  
February 14, 1803.

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XIX. *On the Preparation of the Hyacinthus non scriptus, or common Blue-Bell, as a Substitute for Gum-Arabic.* By Mr. THOMAS WILLIS, of Lime-Street, London\*.

IN the year 1794, whilst collecting plants in a wood for botanical specimens, I observed that the root of the *hyacinthus non scriptus*, the plant commonly called blue-bells, or hare-bells, was extremely mucilaginous; and, on tasting it, I discovered only a very slight pungency. I collected a pound of the bulbs, and, after slicing and drying them before a fire, they yielded about four ounces of powder. I thought that, by keeping the powder some time, the little acridness might go off, as it does in the arum-root powder. I tasted it about six months after, and found it perfectly insipid. I concluded it might be rendered useful for food or nourishment, but at that time pursued the matter no further.

In the spring of 1800, gum-arabic having been a long time very dear, and likely to continue so, I thought this mucilaginous root might answer some of its purposes for external use. I therefore procured seven pounds and a half of

\* From the *Transactions of the Society for the Encouragement of Arts, &c.* vol. xx., who voted a silver medal to the author for his communication.

the bulbs, which, when sliced and dried, produced two pounds of powder. Being soon afterwards in company with Mr. Charles Taylor, secretary to the Society of Arts, &c. I mentioned to him that I had discovered a root which grew in great plenty in this kingdom, yielded a very strong mucilage, and which I imagined would answer the purposes of gum-arabic in some of the manufactories. He said, if I pleased, he would send some of it down to Manchester, to be tried by the calico-printers.

Three or four ounces of the powder were given him, and sent down there: he was informed, upon trial, that it answered the purposes of fixing the calico-printers' colours equally as well as gum-arabic; and in the same proportion, of an ounce and a half of the powder to four ounces of the mordant. Mr. Taylor received the samples of the printed cottons on which it had been used.

On the 15th of January 1801 I furnished Mr. Taylor with eight ounces more of the powder; but have not since heard the result.

As this root can be easily procured, and used at a less price than gum-arabic has been sold for several years past, I think it may be rendered of great utility; and the Society of Arts, &c., by patronizing it, may be the means of making it a public benefit.

Care should be taken, and advice given, that the woods should not be left destitute of the roots; and it would be advisable to offer premiums for cultivating the roots and offsets, as they are very increasing. By such means a constant supply may be had, if the roots answer the intended purposes.

I do not presume to offer any thing respecting the mode in which the society may think proper to divulge the discovery, and promote the use of these roots; but I imagine, that if the roots are bruised and used fresh, they would answer the purpose better than when dried and powdered; and as it is now a proper time of the year for taking them up, and will continue to be so for two months, I wish that the discovery may be made known as soon as possible.

I have sent you specimens both of the dried roots and powder, that they may be seen at the society's rooms by the calico-printers. What I have done have been scorched a little in drying; but the colour would be much better, if proper care was taken in drying them. I am, &c.

Lime-street,  
March 17, 1802.

THOMAS WILLIS.

From the trials made before the committee with this powder with hot and cold water, from samples of the printed cotton

ton produced which had been printed therewith instead of gum-fenegal, and from experiments made in Manchester, it appears that the *hyacinthus non scriptus* may, in many cases, be found a useful substitute for gum-arabic.

XX. On the Edulcoration of Fish-Oil. By ROBERT DOSSIE, Esq.\*

*Explanation of the Principles on which the Purification of Fish-Oil may be performed, and of the Uses to which it is applicable.*

THAT the foetid smell of fish-oil is chiefly owing to putrefaction, it is unnecessary to shew; but, though this be the principal cause, there is another likewise, which is, ustion, or burning the oil, occasioned by the strong heat employed for the extracting it from the blubber of the larger fish, and which produces a strong empyreumatic scent that is not always to be equally removed by the same means as the putrid smell, but remains sometimes very prevalent after that is taken away.

In order to the perfect edulcoration of oils there are consequently two kinds of foetor or stink to be removed; viz. the putrid, and the empyreumatic: and the same means do not always equally avail against both.

The putrid smell of fish-oil is of two kinds: the rancid, which is peculiar to oils; and the common putrid smell, which is the general effect of the putrefaction of animal fluids, or of the vascular solids, when commixed with aqueous fluids.

Fish-oil has not only rancidity, or the first kind of putrid smells peculiar to oils, but also the second or general kinds; as the oil, for the most part, is commixed with the gelatinous humour common to all animals, and some kinds with a proportion of the bile likewise; and those humours putrefying combine their putrid scent with the rancidity of the oil, and, in cases where great heat has been used, with that and the empyreuma also.

The reason of the presence of the gelatinous fluid in fish-oil is this: that the blubber, which consists partly of adipose vesicles, and partly of the membrana cellulosa, which con-

\* From the *Transactions of the Society for the Encouragement of Arts, &c.* vol. xx. So far back as the year 1761 the society voted him a bounty of one hundred pounds for this communication, though they did not publish it till 1802.

tains the gelatinous fluid, is, for the most part, kept a considerable time before the oil is separated from it, either from the want of convenient opportunities to extract the oil, or in order to the obtaining a larger proportion; as the putrid effervescence which then comes on, rupturing the vesicles, makes the blubber yield a greater quantity of oil than could be extracted before such change was produced; and the vesicles of the tela cellulosa, containing the gelatinous matter, being also burst from the same cause, such matter being then rendered saponaceous by the putrefaction, a part of it mixes intimately with the oil, and constitutes it a compound of the proper oleaginous parts and this heterogeneous fluid.

The presence of the bile in fish-oil is occasioned by its being, in many cases, extracted from the liver of the fish; which is not to be so profitably done by other means as by putrefaction; and the bile being consequently discharged, together with the oil from the vessels of the liver containing them, combines with it, both from the original saponaceous property of bile, and from that which it acquires by putrefaction.

This holds good particularly of the cod-oil, or common train, brought from Newfoundland, which, from its high yellow colour, viscid consistence, and repugnance to burning well in lamps, manifests sensibly the presence of bile and the gelatinous fluid; which latter, by the saponaceous power of the bile, is commixed in a greater proportion in this than in any other kind of fish-oil.

A tendency to putrefy, or at most but in an extremely slow manner, is not an absolute property of perfect oils in a simple or pure state, but it is a relative property dependent upon their accidental contact or commixture with the aqueous fluid. This is evident from the case of oils concreted into a sebaceous form; which being perfectly oleaginous and uncombined with any water, except such as enters into their component parts, will not putrefy unless water, or something containing it, is brought in contact with them. But the fluid animal and most vegetable oils being compounded of perfect oils with other mixed substances, either sub-oleaginous or gelatinous, have always a putrescence *per se*, or tendency to putrefy, without further admixture of aqueous moisture. This commixture of heterogeneous matter in fish-oil, particularly of the gelatinous fluid and bile, gives rise to a further principle of purification than *simple edulcoration*, or the removing the fœtor; for the presence of such humours in the oil renders it subject to a second putrescence *per se*, supposing the first corrected; makes it unfit for the purpose of the woollen manufacture,

manufacture, as the heat through which this is in some cases employed causes this matter to contract a most disagreeable empyreuma. It also prevents its burning in lamps, as well from its viscosity as from the repugnance which the presence of water gives to all oleaginous matter. It is therefore necessary to free the oil from this heterogeneous matter; after which it can be subject only to the rancid putrescence, or that which is proper to oils as such.

The substances which have been or may be applied to the removing or preventing the effects of putrescence, are, acids, alkalies, metallic calces, neutral salts, ethereal and essential oils, vinous spirits, water, and air. With respect to acids, though they may be applied with effect to the removal or prevention of putrefaction in mixed animal and vegetable substances, yet they have not the same efficacy when employed in the case of oils; for in a small proportion, without the subsequent aid of alkalies, they rather increase than diminish the fœtor, and in a large proportion they coagulate the oils, and change their other properties as well as their consistence. Though they might therefore be employed with the assistance of alkalies, yet, requiring a more expensive and complex process, and not being moreover necessary, as the same end may be obtained by the use of alkalies only, they may be deemed improper for the purification of animal oils for commercial purposes. Alkaline substances, both salts and earths, are the most powerful instruments in the edulcoration of oils; but as their action on putrid oils, and the method of applying them to this end, are not the same in both, it is proper to consider them distinctly.

Of alkaline salts it is the fixed kind only which are proper to be used for the edulcoration of oils. Fixed alkaline salts, in a dissolved state, being commixed with putrefying animal substances, appear to combine with the putrid matter, and, mixing with some of the principles, form instantly volatile alkaline salts. On the less putrid they seem to act, after their combination, by an acceleration of the putrescent action, till they attain the degree which produces volatile salts. This is evident by the sensible putrid ferment and smell which appear after their commixture; but which gradually abating, the oil is rendered sweeter, much lighter coloured, and thinner.

Their great use in the edulcoration of fish-oil arises therefore from their converting such parts of the gelatinous fluid and bile as are highly putrefied instantly into volatile salts, and causing a rapid putrefaction of the other parts; by which means the oil is freed from them by their dissipation. They  
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do not, however, equally act on the parts of the oil on which the empyreumatic scent depends, unless by the assistance of heat; for when they are commixed with the oils without heat, in proportion as the putrid smell diminishes, that becomes more sensibly prevalent. The ultimate action of lixivate salts on animal oils, except with respect to the empyreuma, seems to be the same either with or without the medium of heat; for the same urinous and putrid smell, gradual diminution of the colour, and foetid scent, happens in one case as in the other, except with regard to the acceleration of the changes; and such salts, where the purification is required to be made in a great degree, are a necessary means, as they are more effectual than any other substance that can be employed.

The use of lixivate salts alone is not, however, the most expedient method that can be pursued for the edulcoration of oils, for several reasons. If they be used alone, cold, in the requisite proportions, they coagulate a considerable part of the oil, which will not again separate from them under a very great length of time; and when they have destroyed the putrid scent, a strong bitter empyreumatic smell remains. The same inconvenience, with relation to the coagulation of part of the oil, results when they are used alone with heat. The super-addition of common salt (which resolves the coagulum and counteracts the saponaceous power of the lixivate salt, by which the oil and water are made to combine) is therefore necessary; and the expense arising from the larger proportion of lixivate salt requires it to be employed if no other alkali be taken in aid, and renders the junction of alkaline earths with it extremely proper in the edulcoration of oils for commercial uses. Lime has also an edulcorative power on animal oils; but it has also so strong a coagulative action, that the addition of a large proportion of alkaline salts becomes, when it is used, necessary to reduce the concreted oil to a fluid state; and therefore this substance alone is not proper for that purpose. The combination of lixivate salt with lime, or the solution commonly called soap-lye, has an effectual edulcorative action on foetid oils; but it makes a troublesome coagulation of part of the oil if no common salt be employed, and must be used in such large proportion, if no alkaline earth be added, as renders the method too expensive.

Lime has a power of combining with and absorbing the putrid parts of the gelatinous fluid and bile when commixed with oil, and effects, either with or without heat, a considerable edulcoration of foetid oils; but it combines so strongly  
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with them, either cold or hot, that the separation is difficult to be effected even with the addition of brine; and the oil, when a large proportion of it is used, can scarcely be at all brought from its concretion to a fluid state but by an equivalent large proportion of lixivate salt: the use of lime, therefore, alone is improper, or even in a great proportion with other ingredients. But when only a lesser degree of edulcoration is required, a moderate quantity, conjoined with an equal or greater weight of chalk, which assists its separation from the oil, may, on account of its great cheapness, be employed very advantageously: it will in this case admit of precipitation from the oil by the addition of brine. It may be also expediently used when lixivate salt is employed with heat for the most perfect purification of oils; for it will in that case give room for the diminishing of the quantity of lixivate salt, though the proportion be nevertheless so restrained as not to exceed what the proportion of lixivate salt (just requisite for the edulcoration) can separate from the oil.

Chalk has an absorbing power similar to lime, but in a less degree, on the putrid substance of oil: it does not, however, combine so strongly with the oil as to resist separation in the same manner, and is therefore very proper to be conjoined either with lixivate salts or lime, as it renders a less quantity of either sufficient, and indeed contributes to the separation of the oil from them.

Magnesia alba, or the alkaline earth, which is the basis of the sal catharticus, and the singular earth which is the basis of alum, both have an edulcorating power on foetid oils, but, like lime, have too strong an attraction with them to be separated so as to admit of the reduction of the oil from the concretion to which they reduce it; and therefore, as they are not superior in efficacy to lime and chalk, but much dearer or more difficult to be obtained, they may be rejected from the number of ingredients that are proper for the purifying of oils, with a view to commercial advantages.

Sea salt has an antiseptic power on the mixed solid parts of animals; but used alone, or dissolved in water, it does not appear to lessen the putrid foetor of oils, but, on the contrary, rather increases it. If after their commixture with it they are subjected to heat, it rather depraves than improves the oils; but though by its own immediate action on them it conduces so little to the edulcoration of oils, yet it is a medium for the separation of water and the alkaline substances requisite to be employed to that end. It is of great utility in the edulcorative processes; for when alkaline salts or earths combine with the water necessary to their action on the oils,

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or themselves form coagulums or corrections with it, a solution of salt will loosen the bond and dissolve the close union; so that the oil being separated will float on the aqueous fluid, while the earth, if any be in the mixture, will be precipitated and sink close together to the bottom of the containing vessel.

Sal catharticus, glauber salt, nitrum vitriolatum, tartar, and other neutral salts, though they counteract putrefaction in the mixed or solid parts of animals, seem to have little effect on oils with respect to their edulcoration, and cannot therefore be ranked amongst the substances proper to be used for that purpose.

Lead reduced to the state of a calx, either in the form of minium or litharge, has a strong edulcorative power on foetid oils, and is indeed applied to that end, with respect to one kind of vegetable oil, for a very bad purpose, considering its malignant qualities on the human body.

In the case of train-oil, which will scarcely ever be considered among the esculent kinds in this country, the same objection against its use would not lie; and employed either with or without heat, it is a powerful absorbent both of the putrid and empyreumatic parts that occasion the foetor.

As, however, there may be some prejudice against its use even in any way, and as it is not absolutely necessary, I have not given it a place among the ingredients of the processes I recommend.

The ochrous earth of iron, commonly called red ochre, has an absorbing power on the putrid parts of oil, but combines so strongly that the separation is tedious even with the addition of brine: if, nevertheless, it is added when chalk and lime have been some time commixed with the oil, as in process the first, it will promote the edulcorative intention, and will subside along with them; and, as it has some advantage without increasing the expense, unless in the most inconsiderable degree, its use may be expediently admitted in that process.

Essential and ethereal oils are applicable to the prevention of putrefaction in the mixed and solid parts of vegetables, but are not so to the edulcoration of foetid oils; and if they had the desired effect, they would not, on account of their price, answer the commercial end, unless the due effect was produced by adding them to the oils in a very small quantity.

The same holds good of spirits of wine as of essential and ethereal oils, both with respect to their efficacy and the expense.

Water has an edulcorative action on foetid oils by carrying off the most putrid parts of the gelatinous fluid or bile, in which,

which, as was above explained, the principal factor resides, if the quantity added be large, and an intimate commixture be made of them by stirring them together for a considerable time: this only partially removing those heterogeneous putrescent substances, the remaining part soon acquires the same state, and the oil again grows foetid, though not to the same degree as before.

Water is, however, a necessary medium for the action of salts and the separation of alkaline earths and calces of metals when they are employed for the edulcoration of oils, as will appear from a consideration of my processes.

Air edulcorates oil by carrying off the most putrid parts, which are necessarily extremely volatile. It may be made to act on them either by simple exposure of them to it with a large extent of surface, or by forcing it through them by means of ventilators, as has been practised by some dealers; but is now, I believe, neglected on account of their finding the improvement of oils by it not adequate to the trouble, as the gelatinous matter and bile, not reduced to a certain degree of putrefaction, being left behind, putrefy again to nearly the same degree as before.

It appears from these several observations, that the cheapest ingredients which can be used for the edulcoration of train-oils are lime and chalk, which may, with the addition of a proper quantity of solution of sea salt or brine, be made to procure a separation of them from the oils, according to process the first, so as to answer for some purposes; that the lixivate salt is the most powerful purifier of oils, and, with the assistance of chalk and brine, will, without heat, according to process the second, effect a very considerable degree of edulcoration; and that lixivate salt used with heat, with the addition of lime and chalk, to save a part of the quantity which would otherwise be necessary, and of brine to procure a quick separation, will perform an edulcoration sufficient for all commercial purposes, according to process the third; but that calcined lead and the ochrous earth of iron may, perhaps, be applied in some cases with advantage, where the oil is not designed for esculent use.

#### PROCESS THE FIRST.

*For purifying Fish-Oil in a moderate Degree, and at a very little Expense.*

Take an ounce of chalk in powder, and half an ounce of lime flaked by exposure to the air; put them into a gallon of stinking oil, and, having mixed them well together by  
6 stirring,

stirring, add half a pint of water, and mix that also with them by the same means. When they have stood an hour or two, repeat the stirring, and continue the same treatment at convenient intervals for two or three days; after which superadd a pint and a half of water in which an ounce of salt is dissolved, and mix them as the other ingredients, repeating the stirring, as before, for a day or two. Let the whole then stand at rest, and the water will sink below the oil, and the chalk subside in it to the bottom of the vessel. The oil will become clear, be of a lighter colour, and have considerably less smell, but will not be purified in a manner equal to what is effected by the other processes below given; though, as this is done with the expense of only one ounce of salt, it may be practised advantageously for many purposes, especially as a preparation for the next method, the operation of which will be thereby facilitated.

## PROCESS THE SECOND.

*To purify, to a great Degree, Fish-Oil without Heat.*

Take a gallon of crude stinking oil, or rather such as has been prepared as above mentioned, and add to it an ounce of powdered chalk; stir them well together several times, as in the preceding process, and, after they have been mixed some hours, or a whole day, add an ounce of pearl-ashes dissolved in four ounces of water, and repeat the stirring as before. After they have been so treated for some hours, put in a pint of water in which two ounces of salt are dissolved, and proceed as before: the oil and brine will separate on standing some days, and the oil will be greatly improved both in smell and colour. Where a greater purity is required, the quantity of pearl-ashes must be increased, and the time before the addition of the salt and water prolonged.

If the same operation is repeated several times, diminishing each time the quantity of ingredients one-half, the oil may be brought to a very light colour, and rendered equally sweet in smell with the common spermaceti oil.

By this process the cod-oil may be made to burn; and, when it is so putrid as not to be fit for any use, either alone or mixed, it may be so corrected by the first part of the process as to be equal to that commonly sold: but where this process is practised in the case of such putrid oil, use half an ounce of chalk and half an ounce of lime.

## PROCESS THE THIRD.

*To purify Fish-Oil with the Assistance of Heat, where the greatest Purity is required, and particularly for the Woollen Manufacture.*

Take a gallon of crude stinking oil, and mix with it a quarter of an ounce of powdered chalk, a quarter of an ounce of lime slaked in the air, and half a pint of water; stir them together, and, when they have stood some hours, add a pint of water and two ounces of pearl-ashes, and place them over a fire that will just keep them simmering, till the oil appears of a light amber colour, and has lost all smell, except a hot, greasy, soap-like scent. Then superadd half a pint of water in which an ounce of salt has been dissolved; and having boiled them half an hour, pour them into a proper vessel, and let them stand till the separation of the oil, water, and lime be made, as in the preceding process. Where this operation is performed to prepare oil for the woollen manufacture, the salt may be omitted; but the separation of the lime from the oil will be slower, and a longer boiling will be necessary.

If the oil be required yet more pure, treat it, after it is separated from the water, &c. according to the second process, with an ounce of chalk, a quarter of an ounce of pearl-ashes, and half an ounce of salt.

*Observations on Process the First.*

This process may be performed on any kind of fish or seal-oil that is putrid and stinking, and will improve it in smell, and generally render the colour lighter, if previously dark and brown: it will also conduce to render these oils fitter for burning, which are, in their crude state, faulty in that point; but it will not meliorate them to the full degree they admit of even without heat, and should therefore be practised when only a moderate improvement is required.

*Secondly,* When the oil is taken off from the dregs and brine, the dregs which swim on the brine should be taken off it also, and put into another vessel of a deep form; and on standing, particularly if fresh water be added and stirred with them, nearly the whole remaining part of the oil will separate from the foulness; or, to save this trouble, the dregs, when taken off, may be put to any future quantity of oil that is to be edulcorated by this method, which will answer the same purpose.

*Observations on Process the Third.*

*First,* This is most advantageously performed on train-oil, called vicious whale-oil; and the more putrid and foul it may be, the greater will be the proportionable improvement, especially if there be no mixture of the other kinds of fish-oils, particularly the seal, which do not admit of being edulcorated perfectly by means of heat, but require other methods: but when the vicious oil is pure from admixture of others, however stinking it may be, the bad smell will be removed by this process duly executed, and the brown colour changed to a very light amber; and these qualities will be much more permanent in this than in any crude oil, as it will not, from the degree of purity to which it is brought, be subject to putrefy again under a great length of time, whether it be kept open or in close vessels.

The oil in this state will burn away without leaving the least remains of foulness in the lamp; and, being rendered more fluid than before, will go further, when used in the woollen manufacture, than any other kind, and will be much more easily scoured from the wool.

If, nevertheless, there be any branches of the woollen manufacture which require the use of a more thick and unctuous oil, this may be rendered so by the addition of a proper quantity of tallow or fat, of which a certain proportion will perfectly incorporate with the oil, the fluidity and transparency being still preserved, as well as all the other qualities that render it suitable to the intended purpose. This may be most beneficially done by adding a proper quantity of the refuse grease of families, commonly called kitchen stuff, which being put to the oil, when moderately heated, will immediately dissolve in it, and let fall also its impurities or foulness to the bottom of the vessel, and render the purified admixture a considerable saving to the manufacturers.

*Secondly,* The different qualities and dispositions of different parcels of vicious oil with respect to edulcoration render various proportions necessary of the ingredients to be used. The quantities stated in the above process are the least which will effect the end in general, and frequently greater will be required; but this may always be first tried: and if it be found, after six or eight hours simmering of the mixture, that no gradual improvement is making in the smell and colour, but that the oil continues the same in those particulars, and remains also mixed with the chalk and lime, and in a thick turbid state, a fourth or third part of the first quantity of pearl-ashes should be added, and the simmering continued

till the oil be perfect. As the quantity of the water is lessened by the evaporation, it is necessary to make fresh additions from time to time; that there may be always nearly the original proportion.

*Thirdly*, If it be inconvenient to give the whole time of boiling at once, the fire may be suffered to go out and be re-kindled at any distance of time; and if, in such case, a small proportion of pearl-ashes dissolved in water be added, and the mixture several times stirred betwixt the times of boiling, it will facilitate the operation. The time of boiling may be also much shortened, if the chalk, lime, and pearl-ashes, be added for some days before, and the mixture frequently stirred.

PROCESS THE FOURTH,

*Which may be practised alone instead of Process the First, as it will edulcorate and purify Fish-Oil to a considerable Degree, so as to answer most Purposes, and for Process the Third, when the whole is performed.*

Take a gallon of crude stinking oil, and put to it a pint of water poured off from two ounces of lime flaked in the air; let them stand together, and stir them up several times for the first twenty-four hours; then let them stand a day, and the lime-water will sink below the oil, which must be carefully separated from them. Take this oil, if not sufficiently purified for your purpose, and treat it as directed in Process the Third, diminishing the quantity of pearl-ashes to one ounce, and omitting the lime and chalk.

ROBERT DOSSIE\*.

XXI. On Painting. By Mr. E. DAYES, Painter.

ESSAY IX.

On Style.

Nor, whilst I recommend the studying the art from artists, can I be supposed to mean that nature is to be neglected. Sir Joshua Reynolds.

**P**ERFECTION in painting requires that the choice, the imitation and execution be directed to the same idea: this only can produce a perfect unity in the whole work. It will appear defective in proportion as it is deprived of a union of these qualities; as that which possesses the fewest

\* The dregs remaining after the sundry processes above mentioned will form an excellent manure, as has been since noticed in Dr. Hunter's Georgical Essays.

faults will ever be the most perfect work. Hence the foundation of a rational inquiry after excellence in art; for he who wishes to examine a picture nicely or critically, must make himself acquainted with the first idea, or general intention of the whole, then of each part separately, considering them in themselves as relative to each other and to the whole.

An inquiry after the different styles of painting would naturally lead to a history of the art itself; but as that is a subject that has been so frequently handled, we shall only touch so far on it as may immediately answer our purpose.

The so much boasted criticisms on the works of the ancients, which are to be found in various authors, can be but little depended on, though often dressed up in the technical phrases of art; because the strongest language will always be used on the most extraordinary occasion, but the advantage resulting to the reader will depend on the capacity of the writer.

All discussion at present on works that have ceased to exist for near 2000 years would be only loss of time, as, from want of reference, they could not tend to illustrate any one rule of art. Such inquiries may serve to amuse the idle, but will not tend in the least to benefit the artist.

No one can deny, however humiliating the thought may be, that we are indebted to the Greeks for some of the finest forms at present in art. Many have attributed their superior powers to the climate: but we ought rather to ascribe them to the freedom they enjoyed, and the wisdom of their legislators; not forgetting their enthusiasm for beauty, which they considered as a gift of the gods; and that with them men were more valued for their personal merit than for the wealth they possessed. The happy Grecian might sit himself down contented in his poverty, knowing it was not thought disreputable, and that his abilities would prove a sufficient passport to society and to the highest honours. The modern artist is solicitous of making money, knowing it is now the first test of his abilities in the opinion of the world, and a stronger recommendation than wisdom. Hence the nobler energies of the soul are weakened; and from want of the stronger motive, honour, the love of fame sinks into the base and selfish desire of wealth. This has induced the weak and pusillanimous to apply to the arts; and as they aspire after nothing but interest, they are unacquainted with those sublime conceptions the arts require.

The Phœnicians are believed not to have carried the arts to any great degree of perfection. To be sufficiently well for the purpose of exportation would be the utmost that would  
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be required; and perhaps their merchants would not be inclined to risk much on articles the buyers of which would be the wealthy, and often the ignorant. What might be requisite for the purpose of ornament would be more likely to be showy than just.

The Romans appear to have encouraged the arts as a means of adding to their pride, and of feeding their vanity. As a people, they are rather to be viewed at a distance; their character is too selfish and oppressive to invite to an intimacy. The best works produced among the Romans from Augustus to Trajan are justly held to be of Grecian workmanship.

It is always more easy to imitate the style than the reasonings and science of the original: hence those who succeeded the early Greeks failed by degrees in the most essential parts; which may account for that sort of mechanical harshness, and often want of elegance, observable in some of the works called Roman.

We have no right to look for true taste among a people who could drag their captives at their chariot wheels, or slaughter them in theatres. They appear to have preferred pomp and show, “barbaric pearl and gold,” to true taste; and exhibit a picture of gaudy and well-disciplined barbarians. A nation which places the first reputation on arms or war, will never have a higher esteem for the peaceable professor of design than a Spartan or Roman. Many of their most renowned works are recommended more to notice on account of their richness than beauty, as Nero’s golden palaces, &c. Pomp and luxury was their object; and they fell into the common error of ignorance, that of augmenting the matter instead of improving the form\*.

Among the moderns, every country that has formed a school appears to have had a cause for its style in its national character. The gravity of the Florentines and Romans might require that justness and truth of form which they saw in the antique statues. The Venetians, a wealthy and gay people, would delight in magnificence from their commerce with the East; and painting for the rich and luxurious would necessarily introduce splendour and brilliancy of colour, with processions, feasts, &c. The Flemish and Dutch were content with superficial or general representations of things. Of the French, the best masters have sought perfection in the Roman school; while others, complying with the national love for splendour, have sought perfection by bustle and show. Of our own nation, the love of locality and portraiture may be

\* In point of order, the Phœnicians should precede the Grecians; but I have paid no attention to chronological arrangement.

said strongly to mark the *amor patriæ*, and to exhibit their charity and love for each other.

A knowledge of the different styles is absolutely necessary to enable us to think deeply and freely. No man, however great his powers, was ever capable of subsisting on his own stock: the more wide the field of inquiry, the more we increase our knowledge, and quicken and enlarge our ideas. We cannot doubt that M. Angelo and Raphael possessed all the knowledge of art discovered in their time. 'Tis our duty to live, as it were, amidst the great works of art, that, by enriching our minds, we may be able to produce something great and noble of our own. This is the true spirit of imitation, and which we may continue, without fear of its doing us an injury, from infancy till we arrive at the fullest state of vigour.

By style as well as manner we are enabled to judge if a picture be the work of a certain master; for, should we remain doubtful from the former, a reference to the penciling may determine the point. For, as in writing or speaking we shall generally discover in persons a fondness for certain phrases, or a peculiar turn or connection of their sentences, by which their style may be known: so in painting we shall discover some favourite part forced on the eye; or thought, attitude, or habit occur, which stamps the master's style. Every one will possess more or less of the style and manner of the master or school from which he comes. Raphael, in his oil pictures, never wholly got rid of that littleness of style derived from Perugino.

To insure a good style we must early habituate ourselves to contemplate noble works of imagination. This, if connected with a vigorous mind, a lively fancy, a strong memory, and good judgment, will be attended with success. That those qualities are necessary to enable us to form a good style, is certain; for by the imagination we conceive images, and if the impressions be clear the style will be so too. But should the images be faint and imperfect, the style will partake of these defects. For it cannot be denied that, as the painter is affected himself, in the same degree will he move the spectator. Hence, if the mind be dull, and indisposed to receive clear and distinct ideas of things, the style will be stiff and heavy; or, if the images be irregular and disordered, the work will be perplexed and confused. It appears to follow of course, that a lively fancy will be accompanied with a happy memory: through this we are supplied with the vast treasures of art and nature; for without a large stock of images we shall never be able to diversify our works in the way variety

riety demands, and they would appear insipid from the too frequent return of the same ideas. But, unless all this be accompanied by a good judgment, the imagination will riot at the expense of reason, and we shall never possess a sound and accurate style. Hence it is that we often confound genius with an active imagination, not recollecting that excess is not its character, and that the more we crowd with incidents the more we weaken; and that, like great talkers, we may be bad orators.

We shall now proceed to describe, in the best way we can, the characters of the various styles. Taste and style differ from each other; the former applying to our choice of objects, while the latter appears to arise from our mode of treating them, or the augmenting certain parts and depressing others, as in the figures of M. Angelo, in which we see the convex lines raised so much as to give them a muscular and gigantic air; by attempting which many of his imitators have often lost the essential character of the object. We say "the great style of Raphael's drapery," not merely on account of the casting of the folds, which shows *taste*, but from his dropping the minute parts, and taking only those essential to the great character in painting. The artists who have sought the great style have pursued the above conduct; that is, have attended to the great and essential character, while those who have forced the trifling parts on the eye have formed a mean or little one. The great style requires the human face, the other includes the wrinkles and other marks of the infirmities of nature.

#### *Sublime Style.*

The sublime style is by many connected with a certain degree of intemperance and excess. The attempts of such may justly be termed the hobgoblin style; for with them nothing is sublime but what will scare a man out of his senses. But scenes terrible or shocking, however admissible in poetry, or where an orator may have occasion to work on the feelings of his audience, do not associate happily with painting; and, when attempted without the greatest care and circumspection, become either ridiculous or disgusting. Brueghel (called hellish), as also Callot, in their whimsical scenes intended to affect us seriously. And Rubens, out of the number of representations he has left of the fall of the damned, has rather shown a warmth of imagination than judgment in their treatment: it is true, the machinery he had to use was human figures; but instead of making the most of them by exhibiting their sufferings, and thereby apply-

ing to our feelings, he has created a set of fiends that make us laugh. Scenes of horror require great delicacy of treatment, as the mind cannot dwell without disgust on representations brought home in that determined way which painting demands. Pilkington relates, from Sandrart, that Spagnoletto represented an Ixion on the wheel so full of pain and agony, that the wife of Mr. Uffel, the possessor, from looking on it when with child, was so affected by it, "that her child when it was born had all the fingers distorted exactly as the fingers of the Ixion appeared in the picture." So sensible of the necessity of this delicacy have the best painters been, that they have concealed as much as possible the shocking parts of a spectacle. In the Slaughter of the Innocents, Poussin would be content with an incident or two, while Le Brun in the same subject has aggravated all the horrors by an endless variety of butcheries. Rubens, in one of the finest sketches of his we have had in this country, has represented, in a disgusting way, a saint with his hands and feet cut off; and has most injudiciously, nay, most insensibly and indelicately, introduced dogs licking up the blood. Contrast the above and Titian's picture of the winding out a saint's bowels on a wheel, with Domenichino's delicate and sensible representation of the death of St. Cecilia. A British artist would display more feeling and delicacy in representing a bull-bait, than either Titian or Rubens have done in the above compositions. Justice must allow that a want of delicacy is not often chargeable on the Italians.

In the small compass we have been obliged to prescribe to ourselves, we fear it will become difficult sufficiently to compress the matter, and at the same time render the subject tolerably clear.

We take it for granted that the mind well-informed is the true standard of whatever is great and illustrious in any point of view. Hence it will follow that in works of imagination we shall derive credit in proportion as we display more or less mental energy. For though we may not possess the power of colouring, or such other excellences as would captivate the eye, yet we may exert the vigour of the soul in the reasoning and science of the work; and this energy it is in our power to improve by education and habit. One of our first duties is to enlarge and elevate our notions; for the dignity and grandeur of our work depend entirely on the dignity and grandeur of our thoughts, and the elevation of the soul. A greater misfortune cannot attend the arts than for men of mean parts to practise them, either as painters or engravers, as they invariably communicate the same bad qualities to their

their works. We may look below the superficies, and not be dazzled with a gaudy appearance, or suffer our admiration to dwell on what the wise would condemn; or we may be led away after pageantry and pomp, mistaking them for true honour and glory. By exerting the mind we may raise our work into the sublime, from a judicious introduction of such accessory circumstances as may add dignity or contribute to the illustration of the story; particularly if they have a skilful connection, and affect the imagination. We have noticed some such instances already in our *Essay on Invention*, as in Raphael's Paul at Lystra, Barry's picture of Elysium, &c. These accessory circumstances occur frequently in the works of the poets, and contribute much to heighten the scene; as in Lear, the thunder storm makes the heart bleed for the sufferings of the old king: and how sublime does the vision of the dagger render Macbeth by its judicious introduction! Vastness is productive of the sublime, by considering any object that takes up much room in the fancy. But we must be careful not to fall into a common error, and mistake greatness of bulk for nobleness in works of art: one of Lyfippus's statues of Alexander, though no bigger than the life, might give the mind more noble ideas than Mount Athos had it been cut into the figure of that hero. This subject has been in some measure amplified already in our *Essays on Invention and Composition*: it may therefore be scarcely necessary to mention, that the sublime requires that the subject of our picture should be a great one, and, if possible, carry with it a universal interest. In the composition, simplicity and gravity are essential to produce grandeur: we cannot wish for better examples than are offered to our view in the Cartoons of Raphael; particularly the Paul at Lystra, his Preaching at Athens, and in the Death of Ananias. We shall observe great grandeur in those fine compositions by West, of the Departure of Regulus, Mark Antony haranguing over the body of J. Cæsar, and the Swearing young Hannibal: the Death of Stephen by the same artist is full of the pathetic: the prints from the above are in every one's hands. Too much contrast destroys the great style; it associates best with the pleasing. Beauty excites gaiety and pleasure, the sublime inclines to seriousness. Elevation of character is also necessary; a want of nobleness contributes much to depreciate the works of the Flemish, and particularly of the Dutch school. The Hercules and the Apollo Belvidere in their form approximate most to the sublime, except we be permitted to conjecture what might have been the famous Jove and Minerva of Phidias. A broad light and shade, or, in other words, a

fine *chiaro-scuro*, add an effect of sublimity. Some of Rubens's pictures strike wonderfully on that account, and Reynolds's Infant Hercules receives an uncommon air of grandeur from the broad and judicious disposition of the masses. Though this style does not require the soft harmonious glow of Venetian colouring, which agrees best with the beautiful, yet it by no means follows, that it authorizes a disregard of the grouping of the colours, to the total neglect of the general harmony of the picture. The colouring should be sober and dignified; we do not mean black and heavy, but composed of such colours as are full, rich, glowing, and rather deep than light. The penciling should be firm and decisive, with the parts well defined. We should particularly guard against trifling events, poor or mean thoughts, and whatever is low and vulgar: such things mar a good whole, and appear worse by contrast, like the blemishes on beautiful bodies. The most esteemed masters have carefully avoided introducing in their works things sordid and base: in the higher style there should never appear any thing insignificant or unnecessary, as dogs, cats, parrots, &c.: such things as these often add a grace to the picturesque, but destroy grandeur. Neither will theatrical splendour or gaudy apparel suit the sublime: the reason is, they affect not beyond the eye; and that which leaves no impression on the mind, we may rest assured, is not the thing we seek. A work truly sublime does not merely please: that is the province of inferior excellence. A grand work will confound, astonish, and, with the impetuosity of a hurricane, bear down all before it. The sublime appears founded on a union of the most noble and elevated parts of nature joined to the most profound efforts of the imagination. Its qualities appear to be simplicity, with a certain uniformity united with solemnity and gravity. Simplicity is necessary to nobleness, as ornament destroys greatness of manner. So the parts should be ample, as is effected by large mantles, &c.

At Agis' summons, with a mantle broad  
His mighty limbs Leonidas unfolds,  
And quits his couch. GLOVER.

M. Angelo and Raphael approach this style in their ideas and inventions, but neither appears to accord with it in his forms. M. Angelo, from attempting the sublime, produced what may be termed the *terrible*, in which the attitudes are forced and extraordinary, and the figures vast, robust, and muscular: he chose in expression the point most extreme, and generally departing from objects in themselves pleasing.

*Expressive*

*Expressive Style.*

In this Raphael stands a shining example, and appears to have touched the extreme point of excellence. Happily we have many of his finest works at hand in the Cartoons at Windsor Castle. Witness his Elymas the forcerer, whose figure is most justly expressive of the punishment of blindness which had befallen him; and the astonishment of the spectators is depicted with great judgment. But let us contrast the above with the Death of Ananias, and observe the consternation and terror in the spectators at a punishment so much more terrible. There we behold with what a masterly hand the artist has raised the feelings in proportion to the exciting cause, without the least waste of force in the expression. In the first picture is more repose; in the second, a greater degree of action corresponding with the interest excited by the event. In the first picture we see the surprise of an earthly judge at an event beyond the power of his understanding; in the other, a calm and dignified set of beings, conscious of the interposition of a Divine Power. Much has been said of the Cartoons; every one speaks of the dignity of the Paul in the picture representing his "preaching at Athens," which for simplicity and dignity reaches the sublime. History is the walk that ennobles the art. This calls forth exertions that elevate and dignify our nature. It might be doubted if the Greek ever arrived to that degree of perfection in expression which Raphael has shown in his works: they certainly sacrificed it to the beauty of appearance. Still we shall look with delight on the Laocoon, the dying Alexander, the Niobes, &c. Raphael, to assist the expression in his figures, appears to have marked the tendons; those who have sought beauty have attended more to the fleshy parts of the muscles. His style of drapery is simply grand, in the extreme of perfection, and well worthy of being studied. Even Pouffin, who is said to excel in it, is in his best works inferior. Raphael is more judicious and select; Pouffin more natural. Justice must allow that the former took the hint for this great style of drapery from M. Angelo. Raphael sometimes reached the graceful, and but seldom the beautiful, and never exquisitely. We have selected the works of the above artist, esteeming them the first for expression; but we shall find admirable traits of the same kind in those of the Caracci, Domenichino, Pouffin, &c. Some who have attempted the expressive style, imagining the whole merit lay in excess and violence, have writhed their figures into convulsions, when the occasion would not require more exertion than

than would be necessary to take a pinch of snuff or raise a straw; nay, they will not let them even sleep in peace. Such bombastic stuff may amuse the weak, as fashion in art will please for a time even at the expense of truth; but the triumph will be transitory as a sun-beam on a winter's day.

### *Beautiful Style.*

Requires that the objects be elegant, void of superfluity, and soft in the execution. Perhaps the truly beautiful style must be sought in the works of the antients. The Apollo Belvidere approaches the sublime, and forms, of its kind, a point of perfection: some of the Niobes exhibit exquisite female beauty, but in the Venus de Medicis and Apollo we see it united with grace. It is certain that in the antique statues we must seek the beauty of symmetry, and that we cannot study them too often to fix the impression of their excellencies on our minds; for it is next to impossible but that every reconsideration must unfold new beauties. The true mode of study is to impress their beauties so strongly on the imagination as never to be forgotten, and not to stand in need of their presence as a pattern. Boys are too often put to draw after the living model before they have imbibed a proper notion of, or relish for, beautiful proportion. There is no danger of such studies injuring, as we are surrounded by fine examples in *chiaro-scuro* and colour; and of the two, decision is more commendable than that slovenly manner which presents the mere idea of a thing like a dream: the former displays knowledge, the latter ignorance. There is great difference in painting, between taking the ideas of natural things without giving them form, and that determined method arising from a positive knowledge. Guido's heads are beautiful; but his choice was often injudicious, many of his objects requiring strong expression, which he lost for fear of destroying beauty. Albano's females are delicately beautiful; and we are surrounded by beauty in the portraits of Reynolds and others.

### *Graceful Style.*

To form this style the motions of the figures should be moderate, easy, agreeable, and unaccompanied by violence\*. The antique statues will assuredly afford the most perfect examples, because, on a comparison with the antients, the moderns appear to have become a little affected, and too often to have sought grace in difficult attitudes, with forced and conceited turns in their figures. There are few of the Greek

\* See the Essays on Grace and Beauty.

statues in which grace does not abound; for, as they sought beauty in their works, they cultivated grace as its inseparable companion. Very graceful are the Apollo and the Venus de Medicis; so are the Meleager and Hermaphrodite. The Hercules is also truly graceful and easy, as is the Antinoüs, &c. Raphael understood the grace of motion; but he had it less in the contours of his figures; and his dry manner of execution contributed much to destroy beauty. Corregio appears the most perfect modern, Parmegiano being sometimes too forced, and bordering on affectation. Reynolds's female portraits appear to have been touched by the Graces themselves. Among the works of our sculptors, the females of Nollekens possess such a combination of grace and beauty as the Greeks might have acknowledged for their own without a blush.

### *Florid Style.*

This style must be sought amongst the Venetians, also in Rubens, and those Flemings who have followed him. Those who have practised it have been content to please the eye by a brilliant display of colour, contrast in their groups, and opposition in light and shade. They appear to have admitted every description of objects in their picture, provided they contributed to the bustle or pleasure of the whole. If this is not the most just style, experience teaches every one that it is the most popular, and perhaps we may add the most profitable.

The above masters must not be confounded with those who have practised a style sufficiently easy, but who have been content with giving such an idea of a thing in their works as would serve to distinguish one from another without regarding their perfection. At the head of this latter class stand P. Cortona and L. Giordano.

### *Natural Style.*

This style is called natural, from the artists practising it wanting the power to improve on the original, or of choosing the best of the kind. It is to be seen in the Dutch school in the works of Rembrandt, G. Dow, Meris, young Teniers, &c. This school has improved the aerial perspective, and possesses great excellence of colour and *chiaro-scuro*, to which it has joined great truth, as far as a simple representation would permit. It has excelled in what may be termed the mechanical part of the art; and those who wish to seek after an elegant power of penciling will be highly gratified in the admirable works of Teniers, Cuyp, Berghem, &c. &c.

As each of the above styles has its peculiar beauties, we must

must cautiously guard against falling into the opposite extreme. For the sublime is nearly allied to the extravagant; the expressive may be easily rendered bombastic or pedantic; and the beautiful, smirking or coquettish. The graceful, with the ignorant, may easily become the affected: perhaps there is but a slight partition between that affectation of grace in the works of Watteau, Bouché, and Le Moine, and the pure representations of Corregio, or those of Parmegiano. The florid may be mistaken for the showy and tawdry. This is most likely to happen from a cold lifeless imitation.

Works of genius are ever the result of feeling, to which we must be directed by spirit and judgment. A painter, a poet, or player, that imitates closely, will never excel. To be tamely alive in our works is not enough; spirit, vivacity, and vigour, are required. Whatever comes short of this is but one remove from imbecility.

XXII. *Memoir on the comparative Anatomy of the Electric Organs of the Torpedo, the Gymnotus electricus and the Silurus electricus.* By E. GEOFFROY\*.

SINCE philosophers have applied with so much success to researches respecting the Galvanic phenomena, it is of more importance than ever that a correct description should be given of the particular organs of certain fishes which are found to possess electric properties.

Analogy gives us reason to believe, that beings which possess faculties so extraordinary are indebted for them to an organization almost entirely similar; consequently, that these beings approach very near to each other, or rather, that they form only one family. But by the examination we are enabled to make, this is not observed to be the case: on the contrary, we are surprised to find that the electric fishes belong to genera exceedingly different, and that they are placed in these genera without violating in any manner the order of natural relations. An electric species, therefore, is known in each of the genera, of ray, tetrodon, trichiurus, gymnotus, and silurus.

To account for the great dissimilarity of the fishes which are distinguished from others of the same genera by the presence of electric organs, we must admit that the latter are not essentially connected with organs of the first importance, and that they belong at most to the common integuments,

\* From *Annales du Muséum National*, No. 5.

which

which vary in each species without producing any remarkable modification in the rest of the organization.

This, however, is a result to which our state of knowledge in regard to those electric organs hitherto observed, does not seem to conduct; for, if we adopt the opinion of most anatomists, it will be found that the electric organ in the torpedo is very extensive, exceedingly complex, and particularly remarkable in this respect, that nothing analogous to it has ever been observed. The Italian school, at the head of which were Redi and his pupil Lorenzini, for a long time considered the numerous tubes of which that organ is formed as so many peculiar small muscles, *musculi falcati*; and this opinion was generally adopted till the late John Hunter published his excellent Anatomical Description of the Torpedo.

During my travels I have also had an opportunity of seeing torpedoes, and I soon distinguished the electric batteries with which they are abundantly furnished. As it was by touching these kinds of apparatus that I experienced the strongest shocks, and as the other rays exhibited nothing of the same kind, I entertained no doubt that I had before my eyes those organs by means of which the torpedo renders itself so formidable in the bosom of the water, and can at pleasure strike with numbness those animals which it wishes to make its prey. But at that time I was ignorant whether others before me had examined this organization; and, in that case, what addition to the observations already made it would be necessary for me to make. Being shut up in Alexandria during the time of the siege, and deprived of the use of my library, I consoled myself for not being immediately able to clear up my doubts, by entertaining a hope that these organs were not known, at least in regard to their relation with general physiology. To obtain that knowledge I endeavoured to find something analogous in the other rays, being persuaded that it was not so much the presence of this organ, as a disposition peculiar to it, that gave the torpedoes alone of all the rays the astonishing faculty of fulminating; as we may say, the small inhabitants of the sea. Those who have compared only a few animals must know that few new organs are found among them, and particularly in species which resemble each other so much as the rays. It was natural therefore to believe, that the tubes containing a gelatinous substance in the torpedo existed in a state of concealment in the other rays; and it will here be seen that I indeed found in the latter an analogous organization, with differences to which we must refer the different modes of existence and action of each species.

Rays,

Rays, as is well known, are flat fish, the pectoral fin of which is prolonged anteriorly on the sides of the head by means of a cartilage that borders its circumference. The torpedo differs from other fishes of the same genus by a very considerable interval between this cartilage and the head. All this large vacuity is filled up by prisms of six, five, and sometimes four planes. These prisms adhere by their bases to the skin above and to that below. They are arranged parallel to each other, follow the projecting and irregular contours of the head and branchiæ, and externally form a semi-elliptic stratum. When the skin is removed, all these prisms, the bases of which are then observed, exhibit the appearance of a honey-comb. They are so many small tubes filled with a substance which by chemical analysis I found to be a compound of gelatin and albumen. The texture of these tubes is aponeurotic, and they are united to each other by a kind of lax reticulation formed of tendinous fibres which envelop them in every direction: in the last place, they are covered and shut by an aponeurotic membrane, and above these coverings the skin is applied. This apparatus is furnished with nerves remarkable for their large size. There are distinguished four principal trunks, which are distributed to all the tubes, and which at length penetrate into their substance and expand in it.

Though rays, in which the cartilage of the pectoral fin immediately borders the contours of the head, were not, like the torpedo, in a condition to exhibit prisms or vertical tubes, they did not differ from them so much as might be expected. In the rays, as well as in the torpedoes, there issues from the cranium, a little before the ear, a nerve so large that it surpasses the volume of that which proceeds to the eye. This nerve proceeds laterally, creeps over the superior face of the masseter, and expands below, between that muscle and the first branchia, in a mass which on the first view might be taken for a gland, but which is really the focus from which proceed, in several bundles, a great number of tubes analogous to those of the torpedo. A bundle proceeds towards the nose, another spreads over the belly, a third ascends on the masseter and terminates behind the occiput, and a fourth extends over the muscles of the pectoral fin. In this respect there are some differences according to the species: but these tubes, in the ray as well as in the torpedo, always adhere to the skin above and to that below; only, instead of being vertical, which is impossible, for want of room, they follow the contour of the head, extend over the most exterior muscles, and are longer according as they have a larger circuit to make

make before they are inserted in the skin. These long tubes seem to be of the same nature as those of the torpedo, and contain a gelatinous and albuminous substance entirely similar. Hitherto we observe in this respect no other differences between the common rays and the torpedo, except that the tubes in the latter are very short, vertical, close to each other, and parallel; while in the other rays they are much longer, bend around the principal muscles of the electric machines, and divide into several bundles formed of divergent radii.

But if these organs do not vary in each species but by a different arrangement of parts, is it not to be apprehended that we may fall into a consequence contrary to the facts observed, and ought we not to suppose that all rays are more or less endowed with the electric properties of the torpedo? Such, indeed, would be the opinion which we ought to form, if these organs were not distinguished by a character on which depend, in part, the astonishing properties of the torpedo. The tubes in the common rays open on the outside of the skin by orifices peculiar to them, and are so many excretory organs of the gelatinous matter which they contain. In the torpedo all these tubes are completely shut, not only by the skin, which has no perforations, but also by aponeuroses which extend over the whole surface of the electric organ. As the gelatinous matter cannot then escape, it is forced to be accumulated in these tubes: hence, no doubt, the greatness of their diameter, and on this account also their number increases at the different periods of life. Valli and Hunter, indeed, found this progressive augmentation. They counted more than two hundred of these tubes in young subjects, from four to five hundred in adult torpedoes, and even twelve hundred in an individual of a large size.

It is to John Hunter, as already said, that we are indebted for the best description of the electric organs of the torpedo\*. Monro, in his *Physiology of Fishes*, has also described the corresponding apparatus found in the other rays; but I flatter myself that I am the first who compared these organs, who proved their identity, and reduced them to the same system of organization. The electric organ of the torpedo is really an organ of touching, furnished with an apparatus as extensive as that of seeing or smelling. The nerves which proceed thither are so large that their volume appeared to Hunter as extraordinary as the phenomena to which they give rise. They suddenly expand in a gelatinous mucus, and nothing impedes their free communication with external bodies.

\* Philosophical Transactions 1773, p. 481.

There can be no doubt that they perform a very considerable part in the electric phænomena. Hunter was of opinion that they are destined to form, collect, and direct the nervous fluid. Their influence however is proved, since it is known that the concurrence of the will of the animal is indispensably necessary for giving shocks. This evidently results from the observations of M. Valis, and from those which I had occasion to repeat myself.

However, since these nerves are found in other rays distributed nearly in the same manner as in the torpedo, it must be allowed that they are not alone sufficient for the production of electricity, and that for this purpose they must also be in a certain relation with the surrounding parts. Do the apertures of the tubes in the rays favour the issue of the nervous fluid? Or, as in the torpedo, do the nerves require a larger quantity of gelatinous substance to enable them to expand in numerous rami, and to become proper for acting with more energy?

In order that we may ascertain how far these conjectures may be founded in truth, let us compare the electric organs of the torpedo, those described by Hunter in the *gymnotus electricus*, and those which I discovered in the *silurus electricus*. These two fishes are so different from the torpedo, that they afford reason to hope that this comparison will throw considerable light on the present question.

The gymnotus belongs to that order of fishes distinguished by the name of *apoda*: it is the genus which approaches nearest to that of the murenæ and eels; consequently they have a very long body, almost cylindric, and similar to that of serpents. Separated from the murenæ because they have no fins on the back or tail, they are distinguished from them also by the great shortness of the abdomen. The anus, indeed, is so near the head that it opens before the pectoral fins. But, on the other hand, the gymnoti have the tail of a most extraordinary length: it is an organ to which all the rest seem to have been sacrificed, and it is rendered lighter by a disposition peculiar to this kind of fish. The air-bladder, instead of being inclosed in the abdominal cavity, extends to the inside of the tail, and is continued to its extremity. It is above this bladder that there is found in the gymnotus electricus a very singular apparatus still more astonishing by its enormous size than by its structure; an apparatus of which there is no vestige in the other species of this genus, and which may be easily known to be the electric organ of the gymnotus.

This organ is formed by the union of a very great number  
of

of aponeuroses, which extend in the direction of the length of the fish, forming so many horizontal strata, parallel, and distant from each other about a millimetre: other vertical laminæ, of the same nature, and much more numerous, intersect them almost at right angles; which produces a large profound reticulation composed of numerous cells of rhomboidal planes. The interior of these cells is filled with an unctuous substance of a gelatinous appearance.

The electric batteries are divided into four distinct masses, two of which are large and two small. Hunter gives to each pair the name of large and small electric organs.

The large organs are situated immediately below the air-bladder and the vertebral muscles: They are of such a thickness that they form alone more than half the volume of the tail. They are divided by a broad partition, which furnishes them with points, to which they are strongly attached. They adhere superiorly to the air-bladder, and to the muscles which accompany it, by a very compact cellular tissue, and they terminate inferiorly in a round edge towards the origin of the osseous pieces which support the radii of the fin of the anus.

The small organs occupy the lower region of the tail. They begin and end nearly at the same points as the large organs; are situated below them, and on each side of the osseous supporters of the anal fin: their general form is that of two long triangular pyramids. The two lateral faces of these pyramids are covered by muscular fibres, the aggregate of which produces the different motions of the fin. In this respect these small organs differ essentially from the large ones which are attached immediately to the skin, and which, having a more intimate connection with external bodies, are capable of producing more energetic effects.

The horizontal laminæ of the small organs, instead of being parallel throughout their whole length, are undulated at intervals. Hunter counted 24 in a large organ, and 14 in a small one. The laminæ which intersect the latter at right angles are far more numerous: they are so thin, and so near each other, that 240 are found in the extent of 9 inches.

This apparatus of electric organs is put in play by a system of nerves furnished by the spinal marrow, and distributed with admirable mechanism. Above the vertebral column is found a large nerve, which proceeds in a straight line from the cranium to the extremity of the tail; but though larger, and nearer the vertebral column, in the *Gymnotus electricus* than in other fishes of the eel kind, it gives out only a few rami, which proceed to the electric organs. On the other hand, however, there issues from each vertebra a nerve, which not

only distributes branches to the muscles of the tail, but sends some also to the electric organs. The different rami of this nerve creep first over the surfaces of these organs, and spread and expand in their alveoli. It is thus that the vertebral nerves, by means of this deviation from their usual route, and of an augmentation of volume, become in the *gymnotus electricus* so many instruments capable of striking with death, or at least with torpor, all those animals which they touch.

The electric organ of this fish being placed under the tail, and in a part which consequently is removed from the principal systems of life, it was difficult to conceive what were the nerves which could proceed thither. The simplest combination was to deduce them immediately from the spinal marrow; and this, indeed, is that which we see realized in the *gymnotus electricus*.

The simplicity of the means employed by nature is observed in a manner still more particular in the *silurus electricus*. This fish differs almost as much from the *gymnotus* as from the rays, and therefore we must expect a quite different organization.

It is, indeed, neither on the sides of the head, as in the torpedo, nor below the tail, as in the species we have described, that the electric organ of the *silurus electricus* is found. It is extended all round the animal; it exists immediately below the skin, and is formed by a considerable collection of cellular tissue, so thick and compact, that on the first view it might be taken for a stratum of lard: but when closely inspected it is observed that this organ is composed of real tendinous or aponeurotic fibres interwoven with each other, and which by their different crossings form a reticulation the meshes of which are not distinctly visible without the help of a magnifying glass. The small cells or alveoli of this reticulation are filled with an albumino-gelatinous matter. They are prevented from communicating in the inside by a very strong aponeurosis, which extends over the whole electric reticulation, and which adheres to it so closely that it cannot be separated without tearing it. This aponeurosis in other respects adheres to the muscles only by a thin cellular tissue of little consistence.

The nervous system, which completes this electric organ, has no more relation with the nervous branches which we examined in the torpedo and the *gymnotus* than the tubes of the latter have with the peculiar covering of the *silurus electricus*. These nerves proceed from the brain; they are the same as those which my friend Cuvier has observed in all fishes to proceed under the lateral line: but these two nerves  
of

of the eighth pair in the *Silurus electricus* have a direction and volume which are peculiar to that species: they descend, approaching each other on their issuing from the cranium towards the body of the first vertebra, which they traverse. They first introduce themselves through an orifice peculiar to each of them, and then issue on the opposite side by one aperture: after reascending they suddenly separate, and proceed under each of the lateral lines. They are then found lodged between the abdominal muscles and the general aponeurosis which extends over the electric reticulation. In the last place, they penetrate beneath the skin by means of large branches, which proceed to the right and left of the principal nerve. These branches are in number 12 or 15 on each side; they pierce the aponeurosis which lines the interior surface of the reticular tissue, penetrate to the centre of the reticulation, and at last expand in it.

The examination of the three electric organs, which I have compared with each other, necessarily conducts us to some interesting results respecting the kind of modification which organs common to all fishes ought to undergo to develop in some species electric properties. We find, 1st, That the part where the electric batteries are lodged is a matter of indifference, as they are diffused all around the *Silurus electricus*, collected in the tail of the *Gymnotus*, and united on the sides of the head in the *Torpedo*. 2d, That no branch of the nervous system is particularly set apart for these organs, since the nerves distributed thither are all different. 3d, That the form of the cells is also of little importance, as this form varies in each species; but in other respects it is found also that the electric batteries, which on the first view we might be tempted to believe to be so different, have however a great many relations with each other, and may be reduced to the same system of organization. This will appear evident, when it is considered that the electric fishes are the only ones in which we find aponeuroses so extensive and so multiplied in their surfaces, with so considerable an accumulation of gelatine and albumen in the cells formed by these aponeuroses, and nervous rami so large and of such a length. It is by the union, indeed, of these simple instruments that the electric organ is constituted; and in this state, according to the judicious remark of my colleague Lapeyrou, it may be compared to the Leyden flask, or an electric picture, since it is alternately composed of bodies which conduct the electric fluid (the nerves, and the albumino-gelatinous pulp to which the action

\* *Histoire Naturelle des Poissons*, vol. ii. Description of the *Gymnotus electricus*, p. 166.

of the nerves is continued), and of non-conducting bodies, such as the aponeurotic laminæ, extended through this mass of albumen and gelatine. What proves that it is on the mechanical arrangement of these idio-electric and non-electric elements that the properties of the torpedo depend, is the existence of the same parts in other rays, though these fishes are not capable of producing the same effects. These parts, similar in regard to their intimate nature and texture, are disposed in a manner entirely different. The nerve of the fifth pair in the rays and squali is of a considerable volume, and expands in a medium from which flows a great quantity of albumino-gelatinous serosity: but this gelatine either is lost on the outside by tubes which open without the skin, or is accumulated in a mass on the sides of the bones of the nose. In the latter case, the gelatine, whatever be its quantity, is of no use for the production of electricity. This, no doubt, must be ascribed to the want of aponeuroses, which divide it into small insulated portions—in the same manner as the Leyden flask, or the electric picture, would fail of their effect if deprived of the glass laminæ interposed between the metallic coatings.

The electric organ, being formed of nerves and aponeurotic laminæ, interlarded, if I may use the expression, with albumen and gelatine, we ought not to be astonished at meeting with it in families altogether different. All animals have nerves which are lost under the skin; all those immediately below it are more or less provided with cellular tissue: all then have, in some measure, the rudiments of an electric organ. If we now suppose that nourishing vessels deposit albumen and gelatine between the leaves of the cellular tissue which fixes the skin to the exterior muscles, we shall easily form an idea of the manner in which this deposition may give rise to the existence of an electric organ. All this may take place without the influence, at least in an immediate manner, of the other organs essential to life. It is a development which takes place almost without the animal, and which has no action but on the skin and parts which depend on it; and hence the reason why species which exhibit alone a development so extraordinary belong, however, to a numerous genus without presenting any striking anomaly.

I have thought it necessary, for the benefit of naturalists who apply to the study of natural relations, to insist on this remark; and I shall terminate this memoir by another, which in my opinion must prove interesting to the learned.

I have reason to believe that the Arabs, at the period no doubt when they cultivated the sciences with so much success,

cess, had approached nearly to the theory of electricity; it is at least probable that they referred to the same cause the fulminating effects of the torpedo, and those, much more terrible, of celestial electricity. We can form no opinion of their knowledge in natural history but by the names which they have given to most of the productions of nature. These names have been preserved without alteration; for it is only to the æra when the sciences flourished in the East that we must refer the rational nomenclature still employed by the rude inhabitants of Egypt. Every animal in that country, as well as in books of natural history, has two names, that of the genus and that of the species. There is no exception but in favour of the torpedo and the silurus electricus. Every thing which related to the form of these fishes has been neglected, and nothing has been attended to but their astonishing faculty of striking with torpor all the small marine and fresh water fishes. Though very different, they have been distinguished by the same name; and, what is very remarkable, this name, *ráad* or *raafsch*, is that used to express thunder. In giving this denomination to the torpedo and the silurus electricus, did the Arabs think of referring the phænomena of animal electricity to celestial electricity?

*Explanation of the Figures, Plate III.*

Fig. 1. the torpedo (*Raia torpedo*).

*a*, electric organ composed of tubes.

*b*, the upper skin turned back on the side to show the electric organ.

Fig. 2. the red ray (*Raia rubus*).

*a, a*, aponeurotic tubes which communicate on the outside of the skin by peculiar orifices.

*b*, the skin of the flanks turned back on the side.

*n*, nerve of the fifth pair.

*i*, focus in which the nerve of the fifth pair expands, and from which proceed, in a radiated form, in several bundles, the tubes which open on the outside of the skin.

Fig. 3. transverse section of the electric eel, *gymnotus electricus*.

*g, g*, large electric organs.

*p, p*, small electric organs.

*v*, air-bladder.

*m, m, m, m*, longitudinal muscles.

*o*, vertebral column.

*d*, skin seen on the outside.

*f*, anal fin.

Fig. 4. *Silurus electricus*.

*b*, aponeurosis which extends over the whole of the electric organ, that is to say, over a reticulation of tendinous fibres comprehended between that aponeurosis and the skin.

*t*, thickness of the electric organ.

*n, n*, nerve of the eighth pair.

*m, m*, abdominal muscles.

XXIII. *On Indian Dogs.* By Dr. BARTON, of Philadelphia.

[Concluded from p. 9.]

IT is highly probable that the Indian dog still exists, in a wild state, in the woods of many parts of North America. It is likely that when seen he has been sometimes mistaken for the wolf.

A very intelligent Indian informed me, that in the year 1792, when travelling towards the head waters of the river Miami, which empties into lake Erie, he had met with wolves which barked like dogs, though in other respects they appeared to be little different from wolves. Perhaps future researches will show that these were the real Indian dogs in their wild state. The subject is worthy of further inquiry. If the Indian dog be an hybrid animal, we ought to suppose that he is less common in the woods than the animals from whom he is sprung. Hybrids are, in general, more rare than original species. This observation applies both to the animal and to the vegetable world.

The late Mr. Peter Kalm informed Mr. John Bartram that the dogs which he saw among the Indians of Canada "were just like the dogs in Sweden, and that they had ears sharp-pointed, and standing up like a wolf's. I can remember perfectly well (continues Mr. Bartram) that when I was a boy the Indians came frequently to our house. Their dogs had sharp-pointed upright ears, and we used to think that they were of the wolf breed. Now, whether the Indians had their dogs from the Swedes, who settled in Pennsylvania long before the English settled there, or whether the Indian dogs were natives of North America, and the same kind as those in the north of Europe and Asia, is well worth inquiring\*."

The fact mentioned by Kalm, and the hint suggested by Mr. Bartram, have sometimes, for a moment, led me to believe that the wolf-like dog of the northern Indians may have been received from the Swedes, who formed a settle-

\* A letter in my possession, dated January 17, 1757, from Mr. John Bartram to Mr. George Edwards.

ment in Pennsylvania early in the 17th century. But a little consideration has compelled me to relinquish this idea: for it is certain that the Indians were in possession of this breed of dogs long before the arrival of the Swedes in America.

We have already seen that the Spaniards found domesticated dogs among the Indians of Florida before the middle of the 16th century, almost one hundred years earlier than the Swedish settlement in Pennsylvania. It is not, indeed, certain that these dogs were of the half-wolf breed; but it is probable that they were. In the year 1585 the celebrated navigator captain John Davis observed dogs "with pricked ears" in the lands about Hudson's Bay\*. It is highly probable that these dogs were a variety of the half-wolf breed of the Indians. Captain John Smith, who arrived in Virginia in 1607, a few years before the Swedish settlement, expressly mentions the affinity of the Indian dogs in that country to wolves. I might easily adduce other instances. These are sufficient for my purpose. They evidently show that the Indian dog existed in America before the Swedes planted their first colony in Pennsylvania: consequently, we have no difficulty in answering Mr. Bartram's first question, which I have stated.

Still, however, Kalm's observation is interesting. It leads us to suspect that the dog of the Indians is common to North America and to the northern parts of Europe. Neither should this circumstance, in whatever light we may view the original of the Indian dog, excite our surprise. If, as very many of the traders and others suppose, this dog is the produce of the wolf and the common fox, his parents existed in the old as well as in the new world. He may have been formed in America by the union of these two animals; or he may have migrated into America from Europe along with many other animals, which, it is highly probable, owe their original to that portion of the globe, or to Asia. For that America has received some of its animals (beside its human inhabitants) from Asia and from Europe, I have very little doubt†.

I conjecture it will be found that the dog of the Greenlanders, mentioned by Crantz‡ and other writers, is only a variety of the Indian dog. And, perhaps, the dogs of the Kalmuc Tartars, which are said to have a great resemblance

\* Forster.

† See *New Views, &c. Preliminary Discourse*, p. 101, 102.

‡ *The History of Greenland, &c. vol. i. p. 74. English Translation, London 1767.* "The Greenlanders (says this author) have no tame beasts but dogs of a middle size, which look more like wolves than dogs. Most of them are white, yet there are some with thick black hair: they don't bark, but growl and howl so much the more."

to the jackal, or schakal, will also prove to be of the same breed. But with me these must remain, for some time, mere conjectures. For I am incapable of giving such a minute description of the external and internal appearance of the Indian dog as would enable the naturalists of Europe to decide a question not the least curious in the zoology of the new world. In another work I hope to be able to give a much more complete account of this animal than that which I now communicate to the public.

I have said that the Indian dog is a much more savage or unreclaimed animal than the common dog which has been introduced into America from Europe \*. This circumstance has not escaped the notice of some preceding writers. But I know of no writer who has deduced from it so important a conclusion as Mr. Zimmermann has done. This truly learned naturalist, after remarking that the Europeans who have visited America have considered the Indian dogs merely as tamed wolves, proceeds to deduce the conclusion, that neither America itself, nor its inhabitants, are so antient as the countries or the people of other parts of the world †.

This conclusion proceeds upon the notion, so ingeniously defended by Mr. Zimmermann, that the dog of the old world is merely the wolf reduced to a state of domestication, and varied, both as to his external aspect and as to his essential qualities or manners, by the influence of climate and other physical causes, during the term of some thousand years ‡. This subject is worthy of some of our attention. I regret, however, that in this place I can only touch it in the most superficial manner. This I shall do under the following seven heads.

I. I may observe, in the first place, that it is by no means probable that the wolf is the sole parent or original stock from whence have proceeded all the numerous varieties of animals which go under the general appellation of dogs. I think it much more probable that these varieties are derived from several different stocks or sources beside the wolf; such as the jackal, the hyæna, different kinds of foxes, &c. &c. In their inquiries into the genealogical history of the dog kind, naturalists, by aiming at simplicity, have only tended to involve the subject in confusion. A comparative view of the internal structure of the supposed parents of the dog kind, and the dogs, has been too much neglected. Some attention,

\* See page 7.

† Specimen, &c. p. 91.

‡ The whole of Mr. Zimmermann's inquiry into the origin, &c. of the dog kind, is well worthy of the attention of the naturalist. See Specimen, &c. § 3. p. 83—98.

however, has been paid to this subject. Essential differences between the structure of the wolf and the dogs (I mean the common dogs of the old world, for I know not that any anatomist has hitherto inspected the structure of the Indian dog of America) have been discovered; differences so essential, that I think they forbid the idea that the wolf and the dog are one and the same species. I do not, however, deny that the pure unmixed wolf has, in some countries, been reduced to the domestic state of the dog. But I think it more probable, that even those dogs, which are most nearly allied to the wolf, are hybrids, begotten between this animal and some other species of the genus.

II. From their agreement in internal structure, it is much more probable that the jackal or schakal (the *canis aureus*) is one of the principal original stocks of the dogs of the old world. Professor Gueldenstaedt has remarked, that the *cæcum* of the jackal “entirely agrees in form with that of a dog, and differs from that of the wolf and fox.” “I may add (says Mr. Pennant, whose words I have been using), that there is the same agreement in the teeth with those of a dog, and the same variation in them from those of the two other animals\*.” Moreover, in his manners the common dog is much more allied to the jackal than he is to the wolf, or to any other animal with which we are acquainted. If then this animal, and not the wolf, be the principal parent of the dog kind, the speculations of Mr. Zimmermann ought to have little weight in establishing the position, that the continents of America are a new creation, and their inhabitants new possessors of the soil. For, I think the form of the Indian dog is very considerably remote from that of the jackal, which is not known to exist in any part of America.

III. In order completely to establish his opinion Mr. Zimmermann should have proved that the dog is certainly derived from the wolf. I have just endeavoured to render it probable that the wolf is not the parent of the dogs of the old world; and I formerly† gave some reasons for believing that the Indian dog of America, notwithstanding the conjectures of Lawson, and other writers, is not the pure unmixed wolf, but an hybrid, begotten between this and some other animal.

IV. But in the old world there are dogs not perhaps more completely domesticated, or, in other words, not more thoroughly deprived of their savage aspect and manners, than are the dogs of North America. Such are the dogs of the Kalmuck Tartars. And who that attentively considers the

\* History of Quadrupeds, vol. i. p. 262.

† See p. 7.  
history

history of the country in which the Kalmucks reside will believe that that country is a new creation? Who will venture to conjecture that the Kalmucks themselves are a new people? Moreover, Kalin's observation would lead us to believe that the Indian dogs are the same (and of course not more savage) as some of the dogs in the north of Europe.

V. Some animals are very easily brought into the domesticated state. Others are domesticated with great difficulty. Perhaps there are some incapable of domestication. If the Indian dog be the offspring of the wolf and the fox, or any other animal, we ought not, perhaps, to wonder that he is still more an *animal sylvestre* than the generality of the dogs of the old world; for both the wolf and the fox are with difficulty tamed. In this inquiry we ought also to remember that the master of the Indian dog is a savage. It may readily be conceived that this circumstance will influence the genius of our animal. Living in the woods, and too frequently badly treated by his master, the dog must often leave the huts of the Indians, and, perhaps, imbibe from his parents, in the woods, a new tincture of their aspect and their manners. Even in our cultivated towns, how much do the manners of the dogs seem to depend upon the calling of their masters! It is a fact, that the dogs of our frontier settlers have a much more savage aspect than the dogs (the same variety) in the villages and populous towns.

VI. In America there were found some kinds of dogs which were not less domesticated than the dogs of the old world. Such were the *alco* and the *itzcuintepotzotli*, of which I have already given some account. I think it very improbable that these two species or varieties were derived from the wolf. Nor is it certain that they were not a species of *canis* essentially distinct from those of the old world. In whatever light we view them, they seem to oppose an objection to Mr. Zimmermann's notion concerning the recent creation of America, and the recent population of this great portion of the globe. Could it be proved that the *alco* and the *itzcuintepotzotli* have sprung from the wolf, it would be natural to infer that an immense period of time had elapsed before these animals could have been brought into the mild, domesticated state in which the discoverers of America found them.

VII. and lastly. This is not the place to inquire into the period of the population of America. I have touched upon this question in another work \*, and shall examine it more fully in a work in which I have long been engaged. Here,

\* New Views, &c. Preliminary Discourse, p. 104—109.

however,

however, I may observe, that many circumstances forbid the idea that America is a new creation, recently emerged from the influence of the ocean. And circumstances, impressive in their nature, render it extremely probable that many of the nations of America have resided in this portion of the world for some thousand years. Trying them by their languages, the Americans will appear to be children of the earliest human families of which history or the traditions of mankind have preserved any memorials.

Among the almost innumerable charges which have been brought against the Indian inhabitants of America, there is one which it becomes the historian of Indian dogs to take some notice of. The Indians are accused of great severity or cruelty in the treatment of their dogs. Mr. Lawson says the savages are the “worst dog-masters in the world, so that it is an infallible cure for sore eyes ever to see an Indian’s dog fat\*.” I have already made mention of this respectable traveller’s notion of the process by which he supposed wolves are turned into dogs†. The faithful father Charlevoix says, the Indians feed their dogs “but poorly, and never fondle them‡.” Carver, who so frequently borrows from Charlevoix, says nearly the same thing§. It is even said, that, owing to their scanty allowance of food, the Indian dogs are often so weak, that they are obliged to lean against a tree, or some other prop, whilst they bark.

It is well known how much ingenuity, eloquence, and science, have, within the last fifty years, been employed to represent the Americans as the degenerated, or imperfectly organized, children of the earth. To complete the large volume of calumny against these poor people, even the manner in which they treat their dogs is not suffered to pass unnoticed by the historians of the new world. “Prior to their intercourse with the people of Europe,” says the eloquent Dr. Robertson, “the North Americans had some || tame dogs, which accompanied them in their hunting excursions, and served them with all the ardour and fidelity¶ peculiar to the species. But, instead of that fond attachment which the hunter naturally feels towards those useful companions of his

\* A New Voyage, &c. p. 38.

† See p. 7.

‡ A Voyage to North America, &c. vol. i. p. 79.

§ Travels, &c. p. 416.

|| They had many tame dogs. The liberality with which the Indians supplied Soto’s men, and the facility with which the men supplied themselves with dogs, leave us no room to doubt that tame dogs abounded in Florida.

¶ Their fidelity has been called in question. See p. 7.

toils, they requite their services with neglect, seldom feed and never caress them \*."

It would, I believe, be a much easier task to prove that Dr. Robertson was unqualified to write the history of America; to prove that the Indian Americans are not the inferiors of the people of the old world in the measure of their intellectual endowments; and to show that more than one-half of the charges which have been brought against these people are charges resulting from ignorance or from systematic zeal, than to prove that the Indians are peculiarly entitled to the character of kind and tender dog-masters. After some attention to this subject, I must candidly confess that I possess not materials for a satisfying defence of the Indian. The charges which have been brought against him by the writers whom I have mentioned will be convictive. But why, in this inquiry, if the historian will condescend to mention the fact, and to interweave it with his eloquence, should he forget the hardships of the savage life? Where the master labours under a scarcity of food, his servants, the animals which depend upon him for their subsistence, must share in the hardships and the evils of his state. The miserable condition of the Indian dogs is a necessary result of the miserable condition of the Indians themselves. This is certain; though the Indians tell us that they keep their dogs poor that they may be light and nimble, and therefore the better fitted for the purposes of hunting.

Dr. Robertson, however, might have found, in the writings of some of the authors whom he has repeatedly quoted, mention made of the tenderness which the Indians manifested towards their dogs in some parts of America. The following passage in Acosta should not have escaped the historian's notice. Speaking of the *alco*, the learned jesuit says: "The Indians doe so love these little dogges that they will spare their meate to feede them, so as when they travell in the countrie they carrie them with them upon their shoulders or in their bosomes, and when they are sicke they keepe them with them, without any use, but only for company†." Hence it appears, that of one species or variety of their dogs, the Indians, in some parts of the new world, were peculiarly careful, and even solicitously tender.

The Wunaumeeh Indians call the dog *allum*, *al-loom*, *mo-e-kan-neb*, and *mē-kan-ne*: the Monkees, *al-lum*: the Mahicans, *dee-a-oo*, *de-a-oo*, and *an-nun-neen-dee-a-oo*: the Chippewas, *a-lim*, *anu-mosch*: the Mellisaugers, *an-nee-*

\* The History of America, vol. ii. p. 216, 217: London 1788.

† The Naturall and Morall Historie, p. 301, 302.

*moosh*: the Ottawas, *an-nee-moo-kau-che*: the Indians of Penobscot and St. John's, *allomooje*: the Natics, *anum*: the Narragansets, *alum*: the Miamis, *aul-la-mo*: the Wiahtannah, *lemah*? the Pottawatameh, *an-ne-moosh*: the Shawnees, *wissi*, *wee-seb*: the Kaskaskias, *remoah*: the Nanticokes, *alum*, and *ibnawállum*: the Mohawks, *abgarijoo*? *er-bar*? the Cochnewagoes, *er-bar*: the Oneidas, *er-bar*, *ale-haul*, *ale-ball*: the Onondagos, *tshierba*: the Cayugas, *fo-waus*: the Senecas, *chee-aah*, and *che-eh*: the Tuscaroras, *cheeth*, *cheeth*: the Wyandots, *nee-a-nooh*: the Sioux, *shungau*, *chonga*, *shun-gush*: the Osages, *shong-eh*: the Cherrake, *keera*, *keethlah*, *keethlegth*: the Creeks, *ee-fa*, *e-fa*, *ef-fa*: the Chikkasah, *o-pbe*, *oo-pbe*: the Choktah, *o-pbe*: the Katahba, *taunt-fee*, *taunsee*, *tase*: the Woccons, *taub-be*: the Natchez, *worfe*: the Mexicans, *chichi*: the Poconchi, *tshi*: and the Chilese, *tewa*.

If the affinity between the Wunaumeeh words, *mo-e-kan-neh*, *mé-kan-ne*, and the Latin *canis*, the Italian *cane*, the Neapolitan *cane*, be not accidental, and who, attentively considering the very many affinities that subsist between the languages of the old and new world, will imagine that it is accidental?), we have probably arrived at the knowledge of the real meaning of the Latin word *canis*. *Mékanne*, in the language of the Delawares, signifies "the barking beast." It appears from Dr. Pallas's great work (*Vocabularia Comparativa*) that certain tribes of Semoyads call a dog *kanang*, *ká-nak*, and *konak*; and that the Karassini call it *kannak*.

#### XXIV. A short Account of the Improvement in Circular Architecture made by Colonel TATHAM.

FOR this invention, which appears to be ingenious, and applicable to many important uses, a patent has been taken out by Messrs. John Scott, James Clarkson, William Tatham, and Samuel Mellish. The invention consists in the form given to the bricks or stones to be employed in circular buildings, which is such as to make them lock, as it were, into each other, and that so effectually as to preclude the possibility of their parting joint by any accident whatever when laid down properly.

Two of the vertical sides of each piece employed in such circular structures answer to two radii from a common centre; a circle from the same centre forms the third vertical side; and the fourth side is formed by another circle of a radius as much

much shorter than the last, whatever that may be, as the thickness intended for the wall: they are, in fact, wedge-formed. This is so precisely the case, that where the pieces (to which the patentees have given the name of *Tatham's clumps*) are intended to form solid pillars, or the like, they exhibit only the three first-mentioned vertical sides.

The horizontal, or under and upper sides of the clumps, have a kind of shouldering or protruding part, and a corresponding hollow or depressed part to fit the similar parts of the next course, and these are at the same time so adjusted that each course will break joint with the one before laid; that is, the vertical joinings of every row fall on the middle of the clumps which form the one below and the one above it.

The form given to the shoulderings or joggelings is so ingeniously contrived, that, were a cylinder formed of these clumps, with the two extreme circles tied together by means of long bolts passing up the centre, secured by screws or other fastenings, it might be laid upon its side and employed as a garden roller, without the possibility of any one of the clumps falling out of its place.

It is easy to see that this contrivance is applicable to a variety of purposes: as,

1st, In rural architecture, &c. For example, in cottages, country houses, villas, circular barns; granaries, secure from rats, mice, and other vermin; feeding houses, cattle sheds, summer houses, castles, towers, turrets, battlements, palisadoe work, door steps, stair-cases (secure from fire), pleasure houses on eminences and near the sea, bathing houses, &c.

2dly, In solid work; as columns, colonnades, agricultural rollers, rollers for roads, garden rollers, stone or brick piers in water works, &c.

3dly, To hydraulic and subterraneous architecture, for sewers, culverts, tunnels, conduits; cylinders in bridge work, thereby giving strength, cheapness, waterway, and lightness, to the structure; mills, reservoirs, communications, wells, cesspools, and the like.

4thly, In marine architecture, to dock work, basins, harbours, piers, jetties, light-houses.

5thly, In military architecture, to magazines, block-houses, redoubts, covert ways, lines of communication, watch towers, &c.

6thly, In architecture applying to warehouses and manufactures; for example, circular windows and other apertures admitting light and air, brewers' vaults, boilers, &c. &c.

7thly, Church architecture; for example, circular chapels,

pels, steeples, columns to support ponderous roofs, the hollow ones with stair-cases when wanted, niches, cells, cloisters, vaults, &c.

*Explanation.*

Fig. 1. *a* is a single clump, having its male point inwards on the side in view; *b* is one with the female point inwards on the reverse of the same clump.

Fig. 2. *a*, one with the male point outwards; *b*, one with the female point outwards. Each of these figures, 1 and 2, represent different kinds of clumps, which being laid, one kind at a time, in a circular form, with the same sides uppermost, will make one course: see fig. 5. for an example of one course of clumps, fig. 1, *a*. Then add one course of fig. 2, fitted in male and female (or joggled); and so on alternate circles, fig. 1 and fig. 2, to the end of the length designed, terminating the same with a cast iron plate, flaunch piece, or key clump, with two male sides so as to admit of taking down for repairs, &c. This method will form a broken joint, such as the one exhibited in perspective view fig. 9.

Fig. 3. is an elongated clump for forming solid columns, colonnades, &c.; *a* having its male point inwards, and *b* having its female point inwards.

Fig. 4. a clump elongated as in fig. 3. *a* shows the view with the male point outwards: *b* the reverse of the same clump, with the female point outwards. These clumps are to be laid into each other in the same manner as fig. 1 and 2, to form a column of solid work, of which a section is represented in fig. 8. If it is intended to construct a circular stair-case with dome-light, &c. on this principle, one or more clumps in each course of work (be the same of brick, earth, stone, or other material, according to the design and extent,) must be of the long kind, to form the *step* of the stairs; all the rest of short clumps, fig. 1 and 2, to admit light and access through the aperture of a spiral ascent.

Fig. 5. *a, a, a*, shows the position of clumps *a*, fig. 1, according to the mode of laying them, with the male point inwards: *b, b, b*, shows the reverse of the same clump with the female point inward. The bottom circle in this and the next figure shows merely the position which a continuation of the clumps would take.

Fig. 6. clumps with blank sides uppermost: these are merely for laying the first course on the ground, or, in some instances, for saving the expense of a cast iron rim next the flaunch of a branch piece. Their reverse must be always indented, or joggled, to fit the next course of clumps. If it is

required to form a flush edge, such as the cheek of a window or door, it can be done by cutting down the clumps into half clumps, as represented by the dotted line X, X, fig. 9. Ravels, rabbits, grooves, &c., and returns of corners, elliptics, &c. must be specifically moulded on the same principle; or girders, ties, &c., may secure any variation by means of suitable cast iron clamps, modified according to the particular contingency.

Fig. 7. a section of a well or pipe, &c. the surface of the circle in view being put together with clumps fig. 2, the male side *a* pointing outwards.

Fig. 8. shows the plan of a solid column formed of elongated clumps of the kind *a*, fig. 4, the male points outwards being in view. As this same modification contracts the circle to a smaller column, in that case fewer pieces will be required.

Fig. 9. is a perspective view, showing a segment of the work (part of a circle) formed out of six clumps laid in their order from figures 1 and 2, the bottom course having a blank side as described fig. 6.

Fig. 10. part of a clump main pipe, showing the manner of flaunching in a cast iron branch piece in order to join the same to a branch main. This can be performed with the help of an iron rim (joggled) as represented in fig. 11; or it can be fitted to blank sides next the flaunch, and the opposite end of the bolt can be counter-sunk into perforated clumps, having the prominent ends fitted with a male and female screw, in order to slide the flaunch piece in or out as occasion demands.

Fig. 11. a cast iron rim indented or joggled to fit a course of clumps as represented on the surface in view, in order to admit the flaunch to slip in and out as required on its smooth surface.

Fig. 12. a plain cast iron flaunch for joining a clump main in suitable lengths to be occasionally taken down: they may also be laid in lengths by means of a key clump, having two male sides to have the joints banded with a hoop.

Fig. 13. shows the mode of fitting the prominent point of the flaunch bolt by means of a male and female screw, so that the flaunch piece may be slid into or out of its birth at option, without hanging on the flaunch plate or joins.

XXV. *On the Freezing Point of Tin, and the Boiling Point of Mercury; with a Description of a Self-registering Thermometer invented by Mr. JAMES CRICHTON, of Glasgow* \*.

SOME time ago, having made some high ranged thermometers, I wished to try their correspondence at high temperatures. I procured for this purpose  $1\frac{1}{2}$  pound of the best effayed tin (block), and, after suspending two thermometers upon a sliding support, I melted the tin in a heat of from  $20^{\circ}$  to  $30^{\circ}$  above the melting point: I then lowered my thermometers down into the metal, and observed the mercury to sink *gradually* in both till it arrived at  $442^{\circ}$ ; then it *instantly* sunk to  $439^{\circ}$ , and as instantaneously rose to  $442^{\circ}$ . At this temperature the mercury remained perfectly stationary for five minutes, at which time the metal became solid to the centre of the crucible. After observing this fact, which to me was new, I waited upon Dr. Mickleham, and requested that he would be present when I should repeat the experiment, to assist me in discovering the fallacy, if there was any. We then procured, from another work, the same quantity of tin as before, and repeated the experiment several times with the same result.

Another accurate standard point, that of  $442^{\circ}$ , for adjusting thermometers, has thus been ascertained; a circumstance which, judging by the use it will be of to myself in constructing high ranged thermometers, I think too important to be withheld from chemists and others who may wish to avail themselves of it.

Before I quit the subject of mercurial thermometers I beg to state another fact which may also be of use to the philosophical world. Quicksilver does not boil at  $600^{\circ}$ , but at  $655^{\circ}$  at the lowest. The precise point I have not yet found leisure enough to ascertain.

I shall now describe a registering thermometer fit for meteorological purposes, which I have lately invented and constructed, and which some of my friends have requested me to make public. Its action depends on the different degrees of expansibility of different metals when exposed to a change of temperature. (See Plate III.)

The length of the instrument is about 13 inches. Fig. 1. represents a front view of it, and fig. 2. a side view: the let-

\* Communicated by the Author.

ters of reference are the same in both, where they can be applied.

The bar A, of which the side BC is of iron, and the other DE of zinc, is about one inch broad, a quarter of an inch thick, and eight inches in length. It is firmly fixed, at the lower extremity I, to a board *a, b, c, d*, made of mahogany.

On the application of heat, this bar, by the superior expansion of the zinc, is inclined at the top towards B. In the upper end of the bar there is a pin (seen at A, fig. 2.) which goes into the fork-like opening L of the index LM (fig. 1.), and carries the short end of this index along with it; consequently the long end is carried along the scale from  $0^{\circ}$  towards  $100^{\circ}$ .

When the instrument is exposed to a lower temperature, the movement is exactly the reverse of what I have been describing.

Beneath the index LM are two others on the same axis G, one of which can be carried towards the right, and the other towards the left, by means of a pin which goes through the first-mentioned index at H. By this means, the greatest and lowest degree of temperature that occurs from the time the thermometer is set or adjusted till it be again consulted, is accurately marked.

In adjusting the thermometer for observation, all that is necessary is to bring the two small indexes immediately below the broad one. As the last-mentioned one is moved backwards or forwards by variations of temperature, it carries one or other of the small ones along with it, by means of the pin H, leaving one of them at the highest and the other at the lowest degree that may have occurred from the last time of adjustment.

The face of the upper part of the instrument is covered with a glass door, the hinges of which are at N and O.

The greatest range of any thermometers of this kind which I have yet made, is from  $10^{\circ}$  below to  $100^{\circ}$  above  $0^{\circ}$ . This instrument proves very accurate in its indications.

XXVI. *Memoir on the Chemical Nature of Ants, and the simultaneous Existence of two Vegetable Acids in these Insects.* By A. F. FOURCROY\*.

ist. SO much has been already said in regard to ants and their acid, that it appears nothing more remains to be added

\* From *Annales du Museum National*, No. 5.

on the subject. Since the time that Samuel Fisher first described this acid in 1670, and taught a method of obtaining it by distillation, till citizen Deyeux confirmed by exact experiments the identity of the acid of ants with the acetous acid, (an identity first announced in 1749 by Margraff, then in 1777 by Ardvissou and Oëhrn, and afterwards by Bergman,) all that chemists have done on this subject seems merely to be, that some of them have maintained the supposed analogy; while others have wished that this analogy should be only apparent, and that the formic acid should be considered as a peculiar acid *sui generis*. I do not speak here of the opinion of Thouvenel, who pretended that the acid of ants was the phosphoric acid, because this opinion was supported by no positive fact: I thought it might be still useful to resume the analysis of ants; and the reader will find that, having been employed in this research with C. Vauquelin, it presented to us some facts which had escaped our predecessors.

2d. Having cleaned the ants which were collected in the wood of Boulogne, and of that kind called by Linnæus *formica rufa*, they were bruised in a marble mortar. During this operation a vapour was disengaged of an odour so sharp and pungent that it hurt the eyes, and could be compared to nothing but the acetic acid or radical vinegar.

3d. The ants thus bruised were put to macerate in alcohol for several days, and in a temperature of about 68 or 70 degrees the alcohol assumed a yellow colour.

4th. The alcoholic infusion of ants subjected to distillation furnished an inflammable liquor slightly acid. During the distillation there was formed in the liquor a brownish deposit which was collected on a filter.

5th. The acid liquor which remained after distillation, when filtered and separated from the deposit, was saturated with lime. It then became brown, thick, and acquired a nauseous and pungent flavour. By blowing through a tube into the thick liquor there was formed a multitude of bubbles, which exhibited the same prismatic colours as soap bubbles.

6th. This combination of the acid of ants with lime, when subjected to the test of different re-agents, exhibited the following phenomena:

a. Mixed with concentrated sulphuric acid it exhaled the odour of vinegar.

b. With nitrate of lead it gave an abundant white precipitate.

c. With nitrate of silver a yellowish precipitate.

d. With muriate of barytes there was nothing sensible.

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e. Ammo-

e. Ammonia produced no change in it.

f. Alcohol formed a rosy and ductile precipitate.

7th. A part of the thick liquor being mixed with half a part of concentrated sulphuric acid and two parts and a half of water, there was immediately formed a very thick magma. This mixture was subjected to distillation, and the product was divided into three parts. They were all clear and colourless: they had an empyreumatic odour, and a very strong acid taste.

The residuum of this distillation had acquired a very dark brown colour, and a disagreeable burnt taste, though it had not been carried to dryness.

The first product tried with acetite of lead gave no signs of the presence of the sulphuric acid: the second and third gave the same result; which proves that they contained no sulphuric acid.

This acid distilled and combined with potash furnished real acetite of potash which attracted the moisture of the air; exhaled, by the addition of concentrated sulphuric acid, a strong vapour like that of radical vinegar; and in a solution of nitrate of mercury formed a flaky precipitate like common acetite of potash.

The greater part then of the acid of ants is acetous acid, as C. Deyeux has already proved by a careful analysis and experiments\*.

8th, The calcareous combination of the acid of ants, obtained by infusing them in alcohol, exhibited to us a very remarkable character. In a solution of acetite of lead it formed a very abundant deposit, which was redissolved by

\* Bergman had found in it some resemblance to the acid of vinegar; the following is what he says on this subject:—*Hoc acidum indole aceto proximè accedit, in variis tamen differunt. Prius cum magnesia, ferro et zinco crystallisabiles præbet sales, posterius nonnisi deliquescentes. Magnesia formicata in primis notatu digna est. In another part of his works, A Dissertation on Magnesia, Bergman speaks also of the formic acid, and describes the properties of the salt which it forms with magnesia as follows:—Acidum formicarum magnesiâ saturatum aquam deferens fundum petit, bundante autem acido solvendum et evaporatione crystallos deponens, quæ saporis fere expertes ægerrime aquâ solvuntur, in igne non funduntur; sed tamen, parum decapitant, subito nigrescunt, tandemque in pulverem fatiscunt album, in acidis effervescentem quum acidum formicarum sub destructione magnesiâ præbeat æream. Crystallorum figura est hemisphærica, plano secante sursum verso, paulum concavo, polito, striisque, oculo armato vix discernendis, è centro radiante: hinc luculenter aceto dignoscitur acidum formicarum, cui alioquin indole proximè accedit, illi tamen, æquè ac acidis vitrioli nitrique phlogisticatis, magnesiâ eripiens hujus salis dimidium pondus est ex magnesiâ. Una ejusdem pars tredecim requirit aquæ, ut solvatur in calore quindecim graduum; spiritus vini eundem non fuscipit.*

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the acetous acid, and even by that which was obtained in the solution above described, No. 7.

As this experiment shows that the acid taken from the ants by alcohol, and combined with lime, contained something else than acetous acid, since this calcareous salt precipitated acetite of lead; and as this different matter was not volatile, since the product of the distillation with sulphuric acid did not produce the same effect, we tried to find it again in the residuum of the distillation: but this residuum contained so much sulphuric acid that it was impossible for us to succeed; we therefore had recourse to the following experiment.

9th. A part of the combination of the formic acid with lime was mixed with a solution of nitrate of lead, and there was immediately formed a very abundant precipitate of a yellowish colour, which when put upon burning coals, after having been well washed and dried, became immediately black, exhaled an animal and ammoniacal odour, while the lead was reduced to a metallic state.

Sulphuric acid diluted with about six parts of water was poured over this precipitate reduced to fine powder. At the moment of mixture the powder remained at the surface of the liquor as if it had been a fat substance, and it did not become mixed with sulphuric acid till after long agitation.

When the mixture was well made the volume of the precipitate was diminished: it became heavier and whiter. The liquor then had a slight acid and saccharine flavour, which was destroyed by the addition of the sulphuric acid, and was soon succeeded by a nauseous flavour. It precipitated slightly muriate of barytes, which indicates some traces of sulphuric acid. It precipitated lime water only very weakly; but at the end of some hours there were formed a great many crystals at the surface of the liquor, and on the sides of the glass containing the mixture.

It precipitated in abundance nitrate of mercury, and those of silver and lead.

Combined with barytes it gave a reddish solution of a saline and pungent flavour. This combination reduced to a small volume did not crystallize; a slight pellicle only was formed at its surface. Potash did not separate the barytes from this combination, but the alkaline carbonate produced a precipitation. The oxalic acid formed in it a very abundant deposit, but the tartarous and citric acids produced in it no change.

10th. The facts here detailed indicate to us that the matter combined with lime along with the acetous acid was of the nature of the malic acid; but to be more certain we examined it comparatively with the latter, combining both of

them with the same substances. Without here entering into a tiresome account of these comparative experiments, we shall content ourselves with saying that these two compounds exhibited phænomena absolutely similar.

Ants contain then, and consequently form, malic acid, like vegetables. It is no doubt the presence of this acid which had misled the chemists who preceded us in this labour. While they acknowledged in the acid of ants a great analogy with vinegar, they however found differences which induced them to consider it as a particular acid; and these differences arose from the presence of the malic acid in the acetous acid of ants.

*Distillation of the Product of Ants treated with Alcohol.*

11th. The ants, exhausted by alcohol, furnished a limpid and slightly alkaline water, a reddish brown oil, thick and exceedingly foetid. The first liquor, diluted with water, and filtered to separate the oil, produced a slight effervescence with acids.

As it emitted at the same time an odour of vinegar, easily distinguished amidst the foetidness by which it was accompanied, a certain quantity of it mixed with sulphuric acid was distilled, and we obtained a colourless acid product of an empyreumatic odour, which contained a small quantity of acetous acid. Was this acetous acid completely formed in the ants exhausted by alcohol, or was it formed by the action of the fire?

The product of the distillation of the exhausted ants contained then a foetid empyreumatic oil, carbonate of ammonia, and acetite of ammonia, all dissolved in a large mass of water.

*Examination of the Matter which separated from the Alcohol during the Distillation mentioned in No. 4.*

12th. The reader will remember that the alcoholic infusion subjected to distillation suffered to be deposited a brown substance which had been separated from it by the filter. This substance was of so dark a red colour that when seen in a large mass it appeared to be black: when dried it was brittle, its fracture was smooth and brilliant like that of resin, it had no sensible flavour, it was not soluble in water; and this may serve to explain its precipitation in proportion as the alcohol evaporated. Though insoluble in water, when macerated a long time in that fluid it communicated to it a slight fawn colour, owing, in all probability, to a small quantity of extractive matter interposed between its parts.

Alcohol

Alcohol slightly heated over this substance dissolved the greater part of it. It assumed a pretty dark red colour: there however remained a brownish matter, which did not combine with that liquid whatever quantity of it was added. This alcoholic solution became milky by the addition of water, and at the end of some days there was separated a resin-like deposit soft and ropy, of a reddish colour and a very disagreeable nauseous taste, slightly soluble in water, since it communicated to it a little of the colour and of the nauseous taste. This deposit is a fat matter of a peculiar nature.

The brownish matter not dissolved by alcohol, already mentioned, appeared to us to be albumen, which the moisture, and perhaps the acid contained in the ants, contributed to render soluble in alcohol. This albumen was coagulated by the heat, and precipitated with the fat matter in proportion as the alcohol was volatilized. When placed on burning coals it decrepitated, became corneous, swelled, and puffed up, emitting white foetid ammoniacal fumes: it left on the support a large and light charcoal. It was in no manner soluble in water; it contained a little hydrogenated carbon, which gave it a brownish colour.

13th. A portion of the ants exhausted by the action of alcohol was subjected to distillation in an open fire. There remained a charcoal, which, like all those of animal substances, burnt with difficulty, and left, after long combustion, white ashes, which contained nothing but phosphate of lime.

This was proved by treating it with nitric acid, which dissolved the greater part of it without effervescence, and by adding to the solution ammonia, which formed in it a white gelatinous deposit which exhibited all the properties of phosphate of lime.

The portion of ashes which did not dissolve in the acid was siliceous; but as the incineration had taken place in an earth crucible, this substance arose, in all probability, from that vessel. However, to ascertain whether this earth had been really furnished by the crucible, some of this charcoal was burnt in small portions in a crucible of platina until we obtained a quantity of ashes sufficient to be subjected to examination, and the following result was obtained:—150 parts of charcoal of ants were reduced in an ignited platina crucible, at the end of several hours, to 22 parts of ashes; 14 parts of these ashes were dissolved by nitric acid; the eight parts not dissolved were sand mixed at first with the ants.

It results from this experiment that the osseous skeleton of ants is formed, like that of warm-blooded animals, of phosphate of lime. It is not improbable that the long and strong calcination

calcination to which it was necessary to subject their charcoal in order to incinerate it, decomposed this salt, and carried it to the state of lime: to confirm this conjecture would have required more of the charcoal than we were able to procure.

14th. It results from this analysis that ants are formed of a large quantity of carbon united to a small quantity of hydrogen, and no doubt also to a little oxygen. This compound is mixed with phosphate of lime, which constitutes the solid part or the skeleton of the insect. Ants, besides the preceding compound, contain a pretty large quantity of resin, soluble in alcohol, which seems to exist in them completely formed, since the application of this re-agent is sufficient to extract and obtain it separately. It is probable that they conceal also some parts of animal albumen and gelatine; but as these animals were subjected to distillation immediately after their treatment by alcohol, it was impossible to obtain the two materials separately.

What ought to be most interesting to chemists among the results obtained by the analysis of ants, is the presence of the acetous and malic acids in these insects. These acids, as appears, exist in them in a very large quantity and in a very considerable state of concentration, since in bruising them in a mortar there is disengaged, as already observed, an acetic acid vapour so sharp and penetrating that it is impossible to endure it even at the distance of three feet. It appears also that acetous acid continually exudes and distills, as we may say, from these animals; for they leave traces of it on the bodies which they traverse. All chemists know, that if moistened turnsole paper be put into an ant-hill, or even if it be suspended at some distance, it soon acquires a red colour. It is known also, that if a certain number of ants be collected in a small quantity of milk it becomes curdled; and that if sugar attacked by ants, and on which they have remained some time, be put into milk, it curdles in the same manner. The acidity of ants is proved besides by the strong impression they make on the mouth when chewed: this impression is almost as strong as that produced by radical vinegar. There is reason to presume that it is this acid in a concentrated state, which by insinuating itself into the places bit by ants renders them so painful, and causes the parts wounded by these insects to swell.

One is astonished on the first view at the quantity of acid which these insects continually furnish, and in particular that they can live in the midst of so sharp a liquid; but it is probable that this acid is separated from the other humours by peculiar vessels which have no communication with the essen-

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tial organs of life, and which open only on the outside of their bodies. Anatomy only can unveil to us this remarkable apparatus of secretion.

15th, In regard to the malic acid which in ants accompanies the acetous acid. We shall terminate this memoir by a general remark on the existence of this acid in organic compounds.

Few vegetable acids are so generally and so abundantly diffused throughout nature as the malic acid; and, though it has not been long known in comparison of many others, it has been discovered in a multitude of substances, and its properties have been carefully studied.

It has been found in fruits with seeds, with stones, and in a great number of berries.

It exists in a multitude of plants in the state of malate of lime. The joubarbes, crassula, and cotyledons, mesembryanthemum, sedum, and even aloes, contain more or less considerable quantities of it, according to the analysis made by C. Vauquelin, and inserted in the *Annales de Chimie*.

It is found in abundance in the liquor which is separated by the hairs of chicken pease (*cicer arietinum*), where it is accompanied with a small quantity of oxalic acid and by some atoms of acetous acid.

It is formed by the action of the nitric and oxygenated muriatic acids on all vegetable substances, and in particular on sugar, gums, starch, honey, &c.: it always precedes the formation of the oxalic acid by these re-agents: vegetable and even animal substances are constantly changed into malic acid before they are converted into oxalic acid, by the acids above indicated.

It is thus that blood, urée, the uric acid, and jelly, when treated with either of the acids above mentioned, are first changed into malic acid, and afterwards into oxalic acid if the action of the acid be continued in a powerful manner; but it is always united with ammonia when it arises from animal matters, because there is formed at the same time a certain quantity of that alkali.

It is not vegetables only that continually give birth to the malic acid: animals are equally susceptible of producing it: it is in the class of insects in particular that this property is manifested.

It appears that the malic acid is in some measure the first stage of acidification in the processes of nature and of art. It precedes in a special manner the formation of the oxalic and acetous acids, because it contains a greater quantity of the radicals, or of carbon and hydrogen, and consequently less

less oxygen than they do. It is that of all the vegetable or animal acids which retains in the fullest manner the nature of the animal or vegetable substance from which it has been formed; it is that which is decomposed with the greatest ease by the action of the fire. Preceding all the vegetable acids; by ulterior and successive elaborations it produces the tartareous, citric, oxalic, and acetous acids: it is by losing a part of its radicals, which are converted into water and carbonic acid by atmospheric oxygen, that it passes itself to the state of these other acids; and though none of the latter but the acetous acid has yet been found in animals, there is reason to believe that all the rest will be found.

The malic acid is then formed by living plants: it often exists in them in its full purity: sometimes it is found in them united to lime, and no doubt to potash, according to the nature of the circumstances which accompany its formation. It is also formed during the life of certain animals, particularly in ants, and certainly in many other insects: in a word, it is produced by the action of the nitric and the oxygenated muriatic acids on animal and vegetable compounds.

Nature then, every time it can dispose of the necessary principles, tends to form malic acid; and there is no reason to doubt, that if plants were examined when very young, at which time they are all acid, the malic acid would always be found present in them. These successive changes afterwards give birth to the other acids already mentioned, and which may be found by further analyses in animals, as it has been found in vegetables.

XXVII. *On the Property which the Acetic Acid possesses of dissolving Camphor and various essential Oils.*

SIR,

*To Mr. Tillock.*

I AM sensible that an apology is necessary for obtruding on you and on your readers a subject which may appear, on first view, to have little claim to general attention. If the rights and privileges of an individual were alone involved on this occasion, I should not have requested a place in your Magazine for the following statement. But it is surely matter of general concern, that the appropriation of inventions and improvements should be dealt, with strict justice, to their authors: for the prospect of this distribution of "honour where it is due" is one of the most animating principles of action; and the extinction of this motive would certainly follow

low an indifference, on the part of the public, to the claims of inventors.

More than fifteen years ago, during the delivery of a course of lectures, by my father, in this town, he had occasion to notice a property of the radical vinegar, or acetic acid, which had not, to his knowledge, been before observed; viz. its property of dissolving camphor and various essential oils. The compound was found to possess a most pungent and agreeable odour; and as the *vinaigre des quatre voleurs* had gained much reputation in obviating infection, it occurred to him that the newly discovered solution would have still more powerful effects in consequence of its high state of concentration. A bottle of this preparation he gave to a late active magistrate and philanthropist (J. B. Bayley, Esq. F.R.S.), who, in the course of an unwearied and undaunted exercise of his public function, was frequently exposed to the danger of foul and infected air. Mr. Bayley was highly gratified with its effects, and not only made constant use of the aromatic vinegar on the bench, and on his visits to the prison, but introduced it to the adoption of several of the judges and principal gentlemen at the bar. He also first suggested to my father the propriety of benefiting by his discovery, and was the medium of a connection with Mr. Bayley, perfumer, in Cockspur-street, London, which has continued to the present day.

The aromatic vinegar, like every other article in general demand, has been a frequent subject of imitation. But it is not of this that I complain; for, in consequence of unremitting attention, our preparation has maintained a decided superiority over all others, both as to quality and extent of sale. The occasion of this appeal to your readers is, that one of these imitations has lately been sanctioned by the name of a respectable physician, who, though not expressly, yet by implication, has conferred on another the credit of that invention, which, in justice, belongs to my father. (See a letter from Dr. Trotter, physician to his majesty's fleet, contained in an advertisement published in the newspapers by a London druggist.)

From the commendatory letter of Dr. Trotter it is evident that he was ignorant of any prior claim; and he was therefore made acquainted by my father, in the most respectful terms, with the facts which have already been laid before you. To this letter the doctor has made no reply, though he declared verbally to a medical gentleman, that my father's preparation had never happened to fall in his way; but that, if it had, he should with equal readiness have given testimony in

in its favour. The advertisement, however, continues to be regularly inserted; and I therefore deem it expedient to appeal thus publicly against such a proceeding; especially in behalf of a man who has imitated the original only in copying, with unblushing effrontery, an advertisement drawn up by myself.

I believe there are few of your readers who will not agree with me, that the ordinary forms of civility required that Dr. Trotter should have taken further notice of the letter addressed to him; that such an attention ought to have been paid to one of the oldest practitioners of medicine in this country; and that more respect was due to a man (whom I trust it is not unbecoming in me to characterize, in terms already publicly applied to him *viris laudatis* \*) “respectable in science and in literature,” and “distinguished by ingenuity, honour, and the strictest integrity.”

I remain respectfully, Sir, your obedient servant,

WILLIAM HENRY.

XXVIII. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them.* By C. LEFEBVRE, Member of the Council of Mines, of the Philomatic Society, &c. &c.

[Continued from p. 76.]

*Department of Ardennes.*

THERE are no coal mines known in this department.

Researches have been made at Etion, but without success. They were begun in schistous strata, which exhibited no indication sufficient to cause the labour to be continued; and the manner in which they were conducted was calculated only to produce loss, since those who directed it followed the course of the schistous strata, instead of traversing them to discover the changes, if there were any, in the order of the strata.

This department receives coals from that of Ourthe, which are conveyed to it up the Meuse.

*Department of Arriege.*

This country (7); abundant in metallic substances, and particularly in iron mines of an excellent quality, possesses no coal mines worked.

C. Vergniesz-Bouischer, proprietor of the forges of Vic-

\* Dr. Aikin and Dr. Percival.

deffos, distinguished by his knowledge and his zeal for improving the iron manufactory, has announced indications of coal at Montesquieu near Foix; and Duhamel, the engineer, has found some also at Mas-'dAzil.

*Department of Aube.*

No coal mines are worked in Aube: the general composition of the strata of the earth, which exhibit only chalk or beds of shells, affords no reason to conceive any hope of discovering collections of this combustible mineral, unless it be at great depths, and after having traversed the whole thickness of the beds of chalk and shells.

It receives no coals but those which are conveyed on the Seine, or which are carried up by La Marne and the river Aube.

*Department of Aude.*

Coals are found in the neighbourhood of Cascastel (8), Quintillan, Ruchan, and the mountains of Fabrezan.

The mines worked near Cascastel, Quintillan, and Segur, furnish about 14000 myriagrammes of fuel per annum.

The products of the other mines are not known, nor do we possess any certain information respecting the price at which the coals are sold. It is probably very low, because there is very little consumption for these mines.

The proprietors complain loudly of the roads, and wish they were repaired; which would facilitate the transportation of coals to Perpignan.

It would be useful also, if it were possible, to provide means for conveying the products of the mines of Fabrezan to the banks of the Aude.

*Department of Aveyron.*

This department (9) is one of the most abundant in coal mines. It is interesting also on account of several other mineral substances, and particularly of those proper for supplying sulphates of alumine and of iron (common alum and green copperas), which are found there in great plenty in the cantons of Milhau, Saint-Affrique, and several others.

The collections of coal known near Cransac, Vialarêts, Livignac, and Montignac, and the neighbouring places on the borders or at a little distance from the river Lot, are inexhaustible, and for the most part can be very easily extracted.

In the year 3 these mines produced above 500,000 myriagrammes, and the price was not more than five cents per myria-

myriagramme. At present it is no more than one cent for the same quantity delivered at the mine, but delivered at Villefranche they cost from 12 to 15 cents.

These coal mines can furnish a long time for an immense consumption, especially if more care were taken in working them. The proprietors of the superficial soil attack them on all sides, and with the greater ease, as the strata and collections of the coal are uncovered, or are generally met with at a small depth; so that the whole of this country presents a multitude of pits begun at the surface, and abandoned when the water or shaking of the ground excites a fear of some accident.

Besides the waste which results from this bad method of working, and the obstacles it creates for the future, the carelessness and negligence of the workmen have given rise in this country to a destructive scourge which daily increases its ravages.

Strata of coal have been set on fire at Fontaignes, Moitot, and in several other places. The conflagration spreads, and is fed even in the bosom of the earth. The calcined surface of the ground for a considerable extent exhibits nothing but the arid and distressing picture of the absence of all vegetation and of the existence of all life.

The places already mentioned in the neighbourhood of Lot, and in the canton of Cransac, are not the only ones in this department where coal mines are found; some are known also in the neighbourhood of Milhaud, on the borders of the Dourbie; at Megamel and Lavergne in the county of Séverac; at Bertholène, and at Sensac in the environs of Rhodéz. A new mine has been opened in the last-mentioned place this year by the care of the prefect.

The annual product of the coals worked in these different places amounts to 220,000 myriagrammes, and might be rendered much more considerable.

C. Saint-Thorent, prefect of Aveyron, has been convinced of the importance of turning to advantage the different kinds of mineral riches with which this country abounds, and he has consequently resolved to carry thither that knowledge and activity, by the help of which this department may become one of the most interesting in regard to the products of national industry. This magistrate had requested that an engineer might be sent thither; and C. Blavier, the person employed for that purpose, soon discovered, not only the existence of several mineral substances never before found in these countries, but the speedy means of working them, and of giving rise to very  
productive

productive establishments \*. The prefect has requested, and obtained permission, for this engineer to be settled at Aveyron, charged merely with the direction of this department.

C. Saint-Thorent has already taken measures to facilitate the communications and to increase the number of the highways in this country, which is in want of sufficient means of circulation. The utility of rendering the Lot navigable for boats above Cahors has not escaped him. The indifference which has been shown, and the delay which has taken place in the execution of this project, must surprise all those who are acquainted with the immense collections of excellent coal which might be dug up on the banks of that river towards Aubin, Livinhac, and Boufquet, and who consider the valuable advantages which might result from establishing an easy mode of conveyance for these coals to the departments of Lot, Lot and Garonne, and as far as Rochelle and Bourdeaux, where the products of these rich mines would banish the use of foreign coals, or at least render the introduction of them unnecessary.

*Department of the Mouths of the Rhone.*

The only part of this department (10) which has hitherto given rise to the extraction of coal is that of the south-east, near to the department of Var. The coal mines are situated in particular in the environs of the communes of Gardanne, Fureau, Tretz, Peynier, Belcodene, Saint-Savournin, Auriac, Roquevaire, and Gemenas.

The greater part of these mines are badly worked, by the proprietors of the ground, or by renters who enter into an engagement with them for permission to dig in their lands. The pits are never carried to a great depth: they are abandoned on the least obstacle which occurs in the course of the labour, which in general is not very safe for the workmen.

These coal mines are found in ground covered by calcareous strata, which are even often mixed with carbonate of lime. The coals are of a moderate quality, especially for the purpose of forges. They do not cake, and form a hollow fire. It appears, however, that the iron masters of the country find means to employ them; but it is only with difficulty.

The quantity of the coal extracted in this country is esti-

\* C. Blavier has discovered mines of copper and of lead, new indications of coal, and, in particular, a very rich and very abundant iron mine, diffused over an extent of more than two chiliometers, in the neighbourhood of the coal mine of Sensac, near Rhodéz. This discovery will give rise to the establishment of founderies, for which the coals will be employed. He has found also considerable depôts of turf in several parts of this department.

mated at about 320,000 myriagrammes annually. The mean price is from 8 to 10 cents per myriagramme delivered at the pits. The principal places of consumption are Aix, Marseilles, and the neighbourhood. They are transported by land carriage in carts.

These coals easily pulverize. They must not be long left exposed to the air. When they have been totally reduced to dust by the action of the atmosphere, or the effect of carriage, they lose almost entirely their combustible property.

The very high price of wood in this country ought to produce much greater care in the working of these coal mines. It is probable that better products would be obtained; and even in case the lower strata should not present coal of the best quality, the longer continuance of the extraction might be ensured.

Subterranean fires have already destroyed a part of the strata of the coals of this country, and particularly at a place called la Galere; at another mine not far distant the strata took fire, and have been burning for several years.

#### *Department of Calvados.*

A coal mine (11) is worked in the commune of Litry, in the canton of Baynes. It furnishes from four to five millions of myriagrammes of coal of the best quality, the greater part of which is consumed in the country. A considerable quantity, however, is exported by the port of Isigny; and this mine, during the war, was a valuable resource to the ports of Cherbourg, Havre, and Honfleur at the mouth of the Seine; and for the manufactories of arms which were put in a state of activity at Saint-Valery-sur-Somme. The price of coals at the mine varies from 12 to 25 cents, according to its quality.

Great praise is due to the renters of the mine of Litry, who use every exertion to give proper activity to this enterprise. In the year 9 they erected there a steam-engine, which exhausts the water and at the same time draws up the coals. This machine was constructed by C. Perrier. It answers the purpose exceedingly well. It saves at this mine eighteen horses daily; and consumes about 50 myriagrammes of coals. It is much to be wished that the useful example set by the renters of the mine of Litry were imitated in other enterprises of a similar kind. I have been assured that the renters of the mines of Anzin, in the department of the North, are going to erect similar engines at their works.

The coal mine of Litry is the only one now worked in the department of Calvados. Searches have been made in several places,

places, but hitherto without success. From the report of C. Duhamel, inspector of the mines, it however appears that those begun at Feugueroles, at a small distance from Caen, deserve to be prosecuted.

*Department of Cantal.*

This department (12), so interesting to natural history, and in particular on account of the antient volcanoes found in it, is not abundant in coal mines. It is only in the north-west, in the district comprehended between Mauriac and Bort, that some collections of this combustible are discovered.

It appears that the small quantity of pits dug are only at the surface, and have been dug in a very irregular manner. We have no accurate information in regard to the quantity of coals extracted, or to the price. These mines, however, which are not far from the course of the Dordogne, and from the place where that river is navigable for boats, might by these means obtain a very extensive consumption, and deserve to be worked with greater care.

*Departments of Charente and Lower Charente.*

There are no coal mines worked in these two departments, and therefore they are obliged to procure their fuel from others. The ports of Rochelle and Rochefort may receive and give circulation to the coals carried thither by sea from the departments of the North or from England, or those conveyed by the internal navigation, and which might be sent to La Gironde by the Tarn, the Lot, and La Dordogne.

*Department of the Cher.*

No coal mines are worked in this department; but it might be supplied in abundance with coals of an excellent quality by the Cher, if that river were rendered navigable with more safety to above Saint-Amand, which would require very little expense. The rich collections of coal of Commentry, and the other strata lately discovered in the environs of Meaulne, in the department of Allier, would obtain a consumption, which is at present wanting; and the working of them would give life to these cantons, and would concur with the products of the valuable forges in the department of Cher to increase the means of industry in that country.

*Department of Correze.*

Coals are dug up in several communes of this department (13), and a great many indications of this combustible are found in it. The communes where the best known mines are

now worked are Argental, where it appears there are abundant collections of coal; La Pleau, where several strata are known, and worked with facility, because they form part of a mountain in which galleries for extracting the coals, and suffering the water to run off, are constructed at a small expense: in a word, the communes of the Cublac, Ventessac, and the environs of Alais.

The annual product of these different mines may be estimated at 50,000 myriagrammes. That of La Pleau supplies the manufactory of arms at Tulle, it affords the principal consumption; and that of Montignac supplies the manufactory of Bergerac.

The Vézère, which begins to be navigable at Uzerche, and La Corrèze at Tulle, furnish some means of transportation for the other mines. The Dordogne, which is navigable for boats only at Souillac, would furnish more means of activity to the mines of La Pleau, and those of Argental, were it rendered navigable as far as the latter.

The mean price of the coals is 10 cents per myriagramme. The mines in general are badly worked, as they want a sufficient consumption.

*Corfica and the Island of Elba.*

The information hitherto obtained in regard to the mineral productions of these two islands does not afford reason to think that they contain coals.

[To be continued.]

XXIX. *Sketch of the General History of Mining.*

SIR,

*To Mr. Tilloch.*

I AM happy in having it in my power to add to the Sketch of the History of Mining in Devon and Cornwall, with which I some time since troubled you \*, a more extended view of the subject from the hands of an ingenious friend. If the insertion of either the former or the present in your valuable work shall ever draw to the subject the attention of those who are able, in different districts, to enlarge our knowledge on this head, it may be a general benefit, and will gratify

Your obedient servant,

JOHN TAYLOR.

Tavistock,  
March 1803.

\* See vol. v. p. 357—365.

*Sketch of the General History of Mining.*

Conamur tenues grandia !

HORACE.

IT gratifies the curiosity, and proves sufficiently amusing, to trace the origin and delineate the progress of any thing which occupies our thoughts or engages our attention ; but when such an inquiry is connected with any important art or science, it surely must be more worthy the prosecution, as being calculated to afford some useful information as well as agreeable entertainment. Such are the effects that seem likely to result from the history of the rise and progress of the art of mining ; and it is hoped that the reader may meet at least with the latter of these effects in this humble attempt to collect into a short compass what may perhaps convey some general idea of the antiquity of the art of working mines, and its gradual advancement to its present dignified and important station.

When we consider the many improvements and important discoveries that have been made in chemistry within these few years, and compare the increase of chemical knowledge in the present age with that of former periods, it may perhaps be thought that we cannot receive any advantages from old authors, and that histories of this kind scarcely deserve notice, because they are not absolutely necessary to the knowledge of mining, nor to the just performance of the several operations belonging to it. Yet, as nothing is more interesting to an artist than the rise and fate of his art, and nothing more useful to him than the labours and experience of his predecessors, it appears incumbent upon us to receive with acknowledgment, and study with attention, the works of those wise and enlightened men in the middle ages who directed their attention to the improvement of the arts, and, being free from the prejudices of hostile sects and attached to the love of truth, contemplated with steady zeal the operations and productions of nature, and described appearances as they existed, without any mixture of theory or hypothesis.

The art of discovering metals in the mine, and rendering them fit for use by chemical processes, is of the most remote antiquity. Moses, the oldest of all authors, mentions the use of brass and iron, which could only have arisen from some acquaintance with the metallurgic art ; and this account is confirmed by the most antient historians and fabulists. Diodorus Siculus relates that the Egyptians adored Vulcan as a god, and esteemed him the first inventor of all arts and operations relating to metals ; and all the profane authors coin-

cide in this belief of Vulcan being the first inventor of the use of metals, and that in the earliest ages of the world. Greece, the first enlightened nation of Europe, received the rudiments of her arts and knowledge from Asia and Egypt, and probably transmitted her knowledge of working mines to the Romans. The antient Greeks and Romans were not deficient in the mathematical parts of mining, as some of their subterraneous works still remain objects of admiration; and we may conclude from their accounts that they were good metallurgists and smelters, though they have left but few traditional practices and processes, and these vague and not to be depended upon.

It seems probable that mines were objects of attention and inquiry among the Romans, as we find that the historian Tacitus, in his account of Germany, remarks that the inhabitants were not acquainted with their present valuable mines of gold, silver, &c.; but doubts whether treasures might not be discovered upon searching for them. “*Argentum et aurum propitii an irati Dei negaverint, dubito. Nec tamen affirmaverim nullam Germaniæ venam argentum aurumve gignere. Quis enim scrutatus est? Possessione et usu haud perinde afficiuntur.*” (*De moribus German.*)

And it seems probable also that the Romans were acquainted with the processes of digging, separating, and purifying, metals; as the mines in Transylvania are generally supposed to be Roman works; and some mines on the Rhine and the Danube, in Lorrain, Alsace, Suevia, and Noricum, appear to have been worked in the decline of the antient Roman empire.

After the destruction of the western empire, when all learning, books, arts, and sciences, were overwhelmed in the general wreck, we have no account of any mineralogical pursuits until the middle of the tenth century, when the mines in the Rammelsburg, near Goslar, and some of the adjacent ones in the Hartz mountains were discovered, and worked to advantage. The metallurgical processes at Goslar were probably conducted upon traditional processes either Roman or German; and we may suppose the knowledge of the Germans was not considerable if we judge from their operations, which, on account of the irony and zincous refractory mixtures, were various, compound, and tedious. The commencement of the sixteenth century may be looked upon as one of the most remarkable and splendid epochs in the annals of mining, as well as in the annals of the world. Arts and sciences then began to be prosecuted with ardour and success, and from this æra the productions of nature were studied and unfolded

to man. The traditional and empirical science of metals now gave way to the scientific principles of chemistry, which were derived from the Arabians.

Agricola was the first of the learned writers on this subject; and for his extraordinary learning and practical erudition in his treatises *De Re Metallica et de Re Fossilia* he is justly entitled to the first place among the chemical metallurgists who have since appeared. H. R.

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XXX. *On the Conversion of Grass Land into Tillage, &c.\**

*To the Editor of the Philosophical Magazine.*

SIR,

WITHOUT offering you any apology for intruding upon your time, I take the liberty of sending you an abstract of a paper to which the utmost consequence is attached from its subject, and written by a man whose name is too well known amongst those skilled in agriculture to need any commendation.

The essay is divided into three chapters, and each chapter is divided into sections. The present communication is an abstract of the first chapter.

Mr. Close sets out in this chapter by saying (which is an undoubted truth) that agriculture is the parent of commerce, but that it has made a very slow progress, considering its utility and the advantages we derive from such a beautiful science. The obstacles which are supposed (and justly too, I firmly believe) by Mr. Close to the improvement of agriculture, are tithes: the difficulty and expense of inclosing waste lands: expensive and injudicious leases: want of knowledge in the practical farmers: the great increase of the poor-rates; and a want of that energy which formerly characterized the agricultural labours of this island.

Mr. Close conceives it would be much more to the advantage of the farmer, and not in the least injurious to the clergy, if some mode of paying the latter were devised instead of taking their income as it is done at present, which is one great check to the improvement of agriculture.

The tithes should be valued throughout the kingdom by some able person or persons, and, after a proper valuation has

\* From a paper on the subject by the Rev. H. Close, of Hordle, near Lymington; published in the Communications to the Board of Agriculture, vol. iii. part 1.

been set upon them, to be offered to the owner at that valuation\*. When the tithes are sold, the money should be invested in the funds; and Mr. Close calculates, that half, or at most three-fourths, of the interest arising from this capital would pay the clergy their present incomes, and the surplus to accumulate for their benefit should provide, &c. advance. In order that the annuitants should receive an adequate compensation if any such advance took place, the average price of wheat should be taken once in four or five years, and a proportionate addition be made to their incomes as the corn had advanced. This measure is proposed by Mr. Cole to originate with the crown. He calculates that the livings now in the patronage of his majesty amount to about 110,000*l.* per annum, to which should be added at least one-third to make their full value, which would come to about 146,000*l.* per annum. If these tithes were sold at only twenty-six years purchase, it would produce a capital of 3,796,000*l.*, which, at only four per cent., would secure to the clergy holding such preferment their full income, and leave above 40,000*l.* a year to accumulate for their benefit should any unforeseen accident arise. By this scheme, neither his majesty nor any other patron of a living would be deprived of their patronage.

To obviate the difficulties attending an inclosure of waste land, Mr. Close proposes, instead of a bill being brought into parliament, that there should be an act "to empower the magistrates at quarter sessions to judge of the expediency or expediency of such inclosure," and that the usual notices should be given of such a measure in the county papers, &c.—The bench of magistrates should appoint five acting magistrates who are entirely disinterested in the inclosure; these five to appoint two commissioners, who shall be sworn to do justice to all parties in adjudging the claims. These commissioners should have a certain sum (first agreed upon) paid them, and by no means to be paid by the day or mile; and not to act upon more than one inclosure at the same time. Should any dispute arise, the magistrates to decide, and the party against whom such a decision is made, to pay all costs†. Mr. Cole observes, that as this is empowering the

\* There are few proprietors, I believe, in this country, from all I am able to learn, who would not jump at such a proposal, were the tithes valued at a price worth his attention.

† It is with great deference I beg leave to differ with Mr. Close respecting his mode of deciding disputes by magistrates: however impartial they are supposed to be, and whatever may be the opinion of the "highest legal authority," it is putting the property of individuals into the power of

the magistrates to decide upon law and fact, he would recommend that a statement be made for the opinion of the "highest legal authority." No right should be preserved to render this inclosure null and void; and should any portion of land be allotted to a person who is the ostensible owner of the estate, and should afterwards be evicted, the common then to be given to the owner. Mr. Cole believes that there is great want of skill, and not, as many suppose, capital.

Of leases, the following sketch he recommends \* :—The landlord covenants to give quiet possession of the house and out-houses, and to put and keep the same in repair; also to put the gates, stiles, and every sort of fence in repair; to insure quiet possession of the above, and of ——— number of acres of lands situated in the parish of ———, for ——— number of years; to pay the land-tax, and allow the tenant to hold certain part of the buildings and lands for a specified term after the expiration of his lease. The tenant covenants to take the premises, to pay his rent in two equal half-yearly payments; to cart all materials for repairs gratis; to find the workmen, when employed in such repairs, the usual allowance of wholesome beer; to keep and leave the gates, posts, rails, and every sort of fence in repair; to allow the landlord or his agents free ingress and regress to shoot, fish, or hunt, over the lands; to warn off every one, and to bring actions, if necessary, at the landlord's expense, against all persons who may trespass upon the premises; not to break up certain portions of meadow and pasture lands; to keep the ditches and water courses open; to crop the lands according to the rotation recommended in the table at the end of this essay †, having respect to the different soils: to pay all parochial taxes; to expend the hay, straw, haulm, and other fodder, produced from the lands, upon the premises; and to use all the dung, soil, and compost, arising from the same, upon the same lands belonging to the premises hereby demised; except the produce of such dung and compost, &c. arising from the two last years' crops previous to the expiration of the lease; all of which shall be left, in a farmer-like manner, in the farm-yards, or other convenient place, for the use

of a set of men whom the old common law of England did never authorize to decide upon such questions, and wresting that invaluable bulwark of our liberties, *the trial by jury*, from our hands, which the constitution does authorize to determine. This blessing is too much curtailed already, and I trust we shall never see it laid aside upon such an important case as the one in question.

\* This sketch appeared to me so important, that I thought it would be better to give it complete, instead of abridging it.

† This will be given in a subsequent paper.

of

of the landlord or in-coming tenant: to leave all the straw of the last year's crop to the in-coming tenant, he carting the corn to any market within the distance of ten miles\*; to pay five pounds an acre additional rent for every acre of meadow or pasture land, specified as not to be broken up, should he plough or till the same; to pay five pounds an acre additional rent for every acre cropped contrary to covenant; to preserve timbers; not to cut, lop, or top, any pollard trees, underwoods, or hedge rows, under eight or ten years growth, according to the productiveness or sterility of the soil: to allow landlord or his agents to cut or fell timber, and to cart the same off the land, at seasonable times.

For the want of knowledge amongst practical farmers, which Mr. Cole thinks very great, he proposes that an experimental farm should be taken by the Board of Agriculture, and to allow a certain number of pupils, on paying a certain sum of money, to be instructed in every branch of agriculture, and also those sciences more immediately connected with it. This farm to be supplied with proper persons as instructors, and particular rules for the conduct of the pupils to be laid down and strictly observed. There is a great want of communication (observes Mr. Cole) with respect to approved facts in agriculture; and being generally known in Norfolk and Suffolk, he proposes that noblemen and gentlemen should take a proper person as a tenant from either of these counties, and place them upon a central part of their estates, lying in a county where the management and improvement of grass lands and stock are well understood. He then gives his opinion from what part of Suffolk or Norfolk these men should be taken, according to the nature of the land upon which they are to be placed.

The poor rates justly come under Mr. Close's censure. He conceives that the enormous rates lately levied are owing to the inadequate price of labour in proportion to the price of provisions†. This not only burthens the farmer with a constant

\* This clause not to extend to dairy farms; as, where a large stock is kept, the out-going tenant should have permission to fodder his cattle with it until the spring, leaving the dung, &c.—*Note by Mr. Close.*

† The following curious circumstance deserves to be mentioned:—When the ministers and their friends in the house of commons were proposing substitutes for bread, and bounties upon different articles, Mr. Horne Tooke, in one of the debates upon a potatoe bill, threw out to the house this very idea of the price of labour being inadequate to the price of provisions, and that the poor man's one pound was not worth one-fourth to him now, when compared with its former value: he recommended to ministers to drop this method of relieving them, as it never would answer,

stant load upon his operations, but falls extremely heavy upon the poor mechanic, whose profession will not permit him to make that advance on his articles the heavy taxes he pays would justify him in doing. It likewise ultimately falls upon the land owner, who, when he lets his farm, the valuation is always taken according to taxes, rates, &c., which the farm pays, and of course a deduction is made in favour of the tenant. It has likewise destroyed a very valuable class of society, viz. the industrious and labouring people, whose honest pride was to boast that they never were beholden to their parish for assistance. The plan of relieving these industrious people is likewise bad; not that it can be altered, but by ascertaining what a man earns, and paying him accordingly: he who is strong and able, and gets a tolerable maintenance, seeing the man who is not able, by reason of his weakness or ill health, receive more, thinks that by slackening his work he shall receive more; and this is absolutely the case almost every where: and he adds, "I am fully persuaded, that by the aggregate body of agricultural labourers not more than three-fourths of the same work is performed as was done ten years back by the same number of hands." Mr. Close then goes on to propose a plan for the relief of what is called "second poor;" viz. those labourers who have never received parish relief, but are entitled to a relief from charities left to parishes. As there are but few who have not had parish relief, and consequently not entitled to these charities, an act of parliament should be passed entitling this class of people to these charities, notwithstanding having received parish relief. To guard against this valuable part of the community being again degraded, he proposes that "the price of labour should be regulated by the price of bread corn." Mr. Close also proposes that those who do receive parish relief after such a regulation as this, should be badged\*.

I am, Sir,

Your very obedient servant,

AGRICOLA.

for they only increased the price instead of lowering it. They saw the wisdom of this remark, and they never proposed any more bounties to lower the price of provisions.

\* Here too, again, I beg leave to differ from Mr. Close with respect to *badging* the poor. It is making a distinction to which the lowest of mankind only will submit, and making a distinction to a class of people who probably may be obliged to seek parochial relief owing to misfortunes, and not to dearness of provisions.

XXXI. *History of Astronomy for the Year 1802. Read in the Athenæum of Paris December 30, by JEROME LALANDE.*

IF we were astonished last year to see the History of Astronomy begin with the discovery of a ninth planet, we ought to be much more so to find this year also a discovery of the like kind, which we did not expect. It was also by a fortunate chance that the tenth planet was found; but it was necessary that chance should favour an assiduous and intelligent astronomer.

At nine in the evening on the 28th of March 1802, Dr. Olbers observed at Bremen Piazzi's planet, which had afforded occupation to astronomers for a year. He was examining with his telescope all the small stars in the Virgin's wing to ascertain their positions; and that he might be enabled to determine with more ease the places of the planet, he had arrived at the twentieth star of Virgo, near which he had observed the planet in the month of January, and was surprised to see near this star, which is of the sixth magnitude, another small one of the seventh. He was certain that it had not been there at the time of his first observations: he therefore hastened to determine its position; and having continued to view it for two hours, he perceived that in this interval it had changed its position. The two following nights afforded him the means of determining its motion, which was  $10'$  per day. As soon as he had made public this curious observation, astronomers took the earliest opportunity of observing this new star, and of calculating its orbit. Dr. Gauss, an able geometrician of Brunswick, calculated the elements, and C. Burckhardt employed himself on similar calculations.

On the 4th of June C. Burckhardt finished his calculations in regard to this star of Olbers. We expected them with great impatience, being desirous to know whether it was a planet or a comet. He found its revolution to be 4 years 7 months and 27 days; its distance 2.785, or 96 millions of leagues; its eccentricity 0.2463, which produces an inequality of  $28^{\circ} 25'$ ; its inclination  $34^{\circ} 51'$ ; its node  $5^{\circ} 22' 28''$ ; its aphelion  $10^{\circ} 2' 3''$ ; and the epoch of its longitude for 1802  $4^{\circ} 23' 10''$ .

On the 14th of November C. Burckhardt published others in the *Moniteur*, which correspond to the observations made as far as the 20th of September 1802. The revolution 4 years 7 months 13 days; distance 2.7699; eccentricity 0.2463; inclination  $34^{\circ} 38' 0''$ ; node  $5^{\circ} 22' 27' 35''$ ; aphelion  $10^{\circ}$

$1^{\circ} 12' 19''$ ; epoch of 1802,  $4^{\text{s}} 23^{\circ} 21' 38''$ , the 31st of December preceding; equation  $28^{\circ} 25'$ . C. Burckhardt has calculated also the perturbations of Olbers's planet, which amount to several degrees; they are long and difficult, and will require changes in the foregoing elements. Dr. Gauss has published in Von Zach's journal for September an ephemeris of Olbers's planet up to the 1st of July 1803. But C. Burckhardt, when he has finished his calculation of the perturbations, will give us elements still more certain.

In the beginning of January Dr. Olbers found again Piazzi's planet, which had disappeared for a long time; and astronomers continued to observe it till the month of July. After the 15th of February Dr. Gauss calculated the new elements; but C. Burckhardt undertook to calculate the perturbations which that planet experiences by the attraction of Jupiter, and found more exact elements. We afterwards received the perturbations calculated by M. Oriani of Milan, and which Dr. Gauss took into account that he might better obtain the elements of its orbit. Those given in Von Zach's journal for the month of November; distance 2.7675; tropical revolution 1681 days 9 hours; eccentricity 0.078835; equation  $9^{\circ} 2'$ ; inclination  $10^{\circ} 57' 37''$ ; epoch for 1803 at Gotha,  $233^{\circ} 37' 35''$ ; aphelion  $326^{\circ} 37' 40''$ ; node  $80^{\circ} 35' 1''$ .

The king of Naples has made an addition of 1200 francs to Piazzi's salary, in consequence of his discovery of the new planet, and the homage he has rendered to his majesty by naming it in his writings *Ceres Ferdinandea*.

This year we have had also a comet, and though very small it was discovered in three places: at Marseilles, on the 24th of August, by Louis Pons, keeper of the observatory; on the 28th, by C. Mechain, one of our most celebrated astronomers, to whom we are already indebted for a great number; at Bremen, on September 2d, by Dr. Olbers, who discovered the tenth planet. This comet was in Serpentarius, very faint and ill defined, having a very sensible nucleus.

C. Mechain and C. Messier at Paris, and C. Vidal at Mirepoix, observed it with care till the 3d of October; and C. Mechain calculated its elements in the following manner from his own observations: Node  $10^{\text{s}} 10^{\circ} 17'$ ; inclination  $57^{\circ} 0'$ ; perihelion  $11^{\text{s}} 2^{\circ} 8'$ ; distance 1.0942; passage of the perihelion, September 9th,  $20^{\text{h}} 43' 15''$  direct motion. The elements calculated by Dr. Olbers, as well as his observations, have been inserted in Von Zach's journal.

It may here be seen that this comet is among the small number of those which at their greatest proximity to the sun  
are

are further distant than the earth: it is the 93d the orbits of which are known.

C. Lalande junior has had the satisfaction of furnishing exact positions of stars, before unknown, with which astronomers had often been obliged to compare this comet; and the same thing has happened to him these fifteen years.

The new tables of the moon by M. Burg form a very important epoch in the history of astronomy for this year. I was informed by Dr. Von Zach that M. Burg had been employed at Vienna, amidst poverty and obscurity, in calculating observations of the moon made at Greenwich, with the hope of improving the tables of the moon: and on the 19th of March 1798 the commissioners of the Institute, being assembled at the Board of Longitude to determine on the subject for a prize, I proposed to them to require the establishment of the epochs of the moon by a great number of observations. I knew that M. Burg had calculated a great many, and I judged that this would give him an opportunity of publishing them, and at the same time afford us the means of rewarding his labour. When the prize came to be adjudged, as C. Bouvard made a great many interesting researches also, it was thought proper to divide it. But general Bonaparte, who that day presided, proposed to double the prize, in order that each might have 3400 francs: and this was agreed to.

C. Laplace, finding that this sum was too small for so immense a labour as that of C. Burg, and that he could deduce from these calculations all the moon's equations with a precision never before obtained, induced the Board of Longitude to propose a prize of 6000 francs, and prevailed on the minister of the marine and of the interior to furnish each the half of this sum. The question was published on the 20th of June 1800, and by the commencement of November 1801 we received the tables so much wished for; and then several supplements. On the 26th of January C. Laplace announced to the Institute, that he had found in the theory of the moon an equation the period of which is 180 years, and which amounts to  $16''$ ; so that it will serve to explain the disagreement observed between the mean motion of the moon 100 years ago and that given by the last observations. This equation is composed of two terms, of which, for the moment, we can have only the sum; but it has served to establish in the motion of the moon, at different periods, a regularity which before could not be obtained.

On the 25th of July a deputation from the Board of Longitude went to give in its report to the first consul on the labours of Burg, and the prize of 6000 francs. I reminded him,

him, that he had caused the first prize to be doubled, and that it would be worthy of his character to double the present one. He instantly complied. The minister Chaptal, who was present, proposed to me to induce Burg to come to Paris, where he should have a pension of 3000 francs. He would be an excellent co-operator in improving astronomy in France, where it is already so much cultivated: but this worthy astronomer preferred his own country with less advantage. His tables of the moon are about to be printed, as well as the new tables of the sun by Delambre; and we have already communicated them to Dr. Maskelyne, the astronomer royal of England, who will be enabled by them to improve the Nautical Almanac, which since 1767 has been of the greatest utility to navigators.

The report of C. Delambre will be published in the *Connaissance des Temps* for the year 13, which is about to appear.

The Arabic text of all the observations in the manuscript of Ibn Iunis, lent to us by the Batavian republic, with a translation by C. Caussin, and an extract from the part which it was not judged necessary to translate and to print, has been finished at the printing-office of the republic. C. Caussin is the first of all the professors of Arabic who has rendered his knowledge really useful. I reproached them fifty years ago, on account of their translating nothing but romances.

The observation of the eighteenth transit of Mercury over the sun's disc on the morning of November 9 perfectly succeeded. We were the more interested in it, as it will not be again seen at Paris till the 5th of May 1832. It confirmed, in the completest manner, the exactness of my tables of Mercury. The egress of the centre, according to a mean of all the observations, took place at 7<sup>h</sup> 34<sup>m</sup> 17<sup>s</sup> 9<sup>''</sup> 9<sup>''</sup>, at 21<sup>h</sup> 2<sup>m</sup> 40<sup>s</sup> 40<sup>''</sup> mean time of the conjunction, even taking into account the correction to be made in the tables of the sun, which by my nephew Lalande was observed to be — 10<sup>''</sup> 4; the geocentric latitude in conjunction 56<sup>''</sup>. I have had the most satisfactory confirmation of the theory of Mercury, which I gave in the first memoir, read in the first sitting of the first class of the Institute on the 1st of January 1796. More details on this subject will be found in the Memoirs of the Institute.

M. Casella, physician to the king of Naples, and M. Bygge, astronomer royal of Copenhagen, &c. have sent me exact observations of this transit.

The solstice was exactly observed with whole circles by C. Delambre, and by C. Burckhardt and Lalande junior.  
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The mean of four hundred observations gives  $23^{\circ} 28' 7''$ , or  $7''$  more than in my tables: last year it was only  $6''$ ; we ought to be satisfied with this agreement.

Duc-la-Chapelle has published, in the fourth volume of the Memoirs of the Institute, solstitial observations which give him  $31''$  for the secular diminution of the obliquity of the ecliptic. C. Mechain has still found 10 seconds less for the obliquity of the winter solstice.

C. Vidal has sent in his observations of the new planets and a great many others: he has even had the complaisance to terminate some zones of circumpolar stars which were not complete in the *Histoire Cœleste Française* published in 1801.

The inferior conjunction of Venus, observed in the end of December by C. Burckhardt and Lalande, gave us for the correction of my tables  $+ 29'$ , which do not make 10 in the longitude seen from the sun.

But as the tables of Venus had not yet been calculated with the perturbations, and as C. Lalande junior proposed to undertake this labour, C. Chabrol furnished him with a table of the equation calculated to tenths of seconds, which was an essential preliminary. It is time it should be begun. We have 40 years exact observations of Venus; and these 40 years observations, from 1761 to 1801, will give us the motion of Venus as exact as the Babylonian observation made 2072 years ago, and respecting which there is some doubt, as I have already explained. (See *Mémoires de l'Académie* 1785, p. 250.)

The opposition of Mars, which took place on the 24th of December, could not be observed; but the observations of that night gave a correction in the tables of Lalande junior  $- 5''$ ; which make only two in the heliocentric longitude.

The disappearance of Saturn's ring, which will take place in 1803, has been preceded by a singular phenomenon. The *anses* were on the point of disappearing, and it was with difficulty that C. Mechain discovered them on the morning of December 20. C. Flaugergues also lost sight of them on the 16th. But we shall soon see them re-appear, as the earth will return on the 28th of June: the sun passing through the plane of the ring, we shall lose sight of them till the 23d of August, at which time the earth will pass to the north of the ring, and permit us to see the surface of it illuminated by the sun. *Dufajour, Traité analytique des Mouvements Cœlestes*, vol. ii. p. 155.

C. Chabrol and C. Flaugergues have calculated a great number of tables of aberrations and nutations for 600 stars of the fundamental catalogue which Lalande junior inserts every

every year in the *Connoissance des Temps*, and always with new improvements.

C. Lalande, my nephew, continues to observe the right ascensions and declination of a great number of stars not well known; and madame de Lalande continues the reductions, which she promised, for the 50,000 stars: 1500 will be found in the *Connoissance des Temps* for the year 13, which will soon appear; and M. Bode has published more than 10,000 in the catalogue of 17,000 stars which accompany the large and beautiful atlas in twenty sheets which I have already announced.

Last year I gave an account of the labour undertaken by the Swedish astronomers Svanberg, Olverbom, Olmquist, and Palander, to verify the degree of the meridian under the polar circle. M. Melanderhielm, though 76 years of age, set on foot and directed this enterprize. In 1801 they had already discovered the stations, elevated the signals, and built two observatories. They set out in the month of January 1802, and measured the base on the ice of the river Torneo between the 6th of February and the 8th of April, though the cold was  $24^{\circ}$ . At the beginning of September they had completed the measurement of the angles of their triangles, and set out to proceed northwards to Pathavara to commence their astronomical observations. We shall therefore soon have the curious result of this new measurement.

On the other hand, C. Mechain, in consequence of the consular decree of September 17, is about to resume the measurement of the meridian as far as the island of Cabrera, which is 40 leagues south of Barcelona, agreeably to his wish, which I announced in the *Connoissance des Temps* for the year 10. By these means the 45th degree, which it is particularly interesting for us to be well acquainted with, will be a mean of the whole interval.

The Portuguese, whose silence we regretted, begin to distinguish themselves. M. Demonfort has sent us calculations of eclipses of the sun visible at Lisbon during this century: M. Monteiro de Rocha, new tables of Mars, with all the perturbations. The equation is  $10^{\circ} 41' 39''$ , greater only by  $4''$  than in the tables of C. Lalande junior. M. Damoiseau, captain-lieutenant of the royal brigade of the marine at Lisbon, has written to me that he is employed on the Nautical Ephemerides of 1806. Those of 1805 were calculated directly, without employing the Nautical Almanac. I have requested him to wait for the new tables of the sun and moon, which are about to be printed.

We have received also a description of the observatory of  
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Coimbra, from which it appears that it is pretty well furnished with instruments: a sector of 10 feet, a 5-foot meridian telescope, a 3 $\frac{1}{2}$ -feet quadrant divided by Troughton of London.

We have received the Astronomical Ephemerides of father Cossali, of Parma: they are not destined for astronomers such as those of Berlin, Vienna, Milan, Bologna, and Paris; but the author has added a very minute history of the two new planets, and he follows my example in giving them the names of Piazzzi and Olbers; he also employs that of Herschel, as a mark of homage due to them by astronomers.

On the 22d of June 1802, M. Van Swinden sent us a Dutch memoir on the new measures, which engaged him a long time; and he has obtained a decree for establishing the new measures in the Batavian republic.

Mr. Mackay has published a curious work, in England, on the longitude.

M. Van Swinden has sent us the fourth edition of his Dissertation, in Dutch, on the Determination of the Longitude by the Observation of the Moon's Distance from the Stars; with a dissertation, which he published in 1788 conjointly with M. Nieuwland, on the Use of Sextants and Octants. He proposes to publish also a memoir on reflecting circles, which he wishes to introduce into the Dutch navy; and also on marine time-keepers. So early as the year 1774 I went to Holland to solicit the introduction of astronomy into navigation, and obtained a promise for that purpose from the stadtholder and the grand pensionary. My Astronomy was even translated into Dutch: but this circumstance was attended with little advantage at that time, notwithstanding the need which navigators had of it. At present, since the navy resumes new activity in the Batavian republic, and that the learned professor enjoys there a well merited influence, we have reason to believe that astronomy will be employed there in an effectual manner.

M. Van Swinden explains in this work all the methods, by calculation, by graphic operations, and by instruments; the corrections made by Mr. Mackay to the methods of Borda and Dunthorn; those of Kraft and of Douwes de Steinstra. He has added a collection of such tables as are necessary in navigation.

M. Mendoza has already published two large collections of tables for navigation: he has added a new method of determining the latitude by two altitudes taken out of the meridian, the calculation of which is shorter than the first approximation, which is only the commencement of the method of Douwes.

We have received the Ephemerides of Vienna for 1803, which contains the new tables of the moon by M. Triesnecker; but it is evident that he has seen those of M. Burg already mentioned: also the Ephemerides of Berlin for 1803, where M. Bode has collected 170 pages of observations on the new planets, and on other important points in all parts of Germany.

M. Schroeter has published a continuation of his observations on the spots of the moon, in a large volume in quarto, entitled *Selenotopographische Fragmente*; that is to say, Topographical Fragments respecting the Moon: to serve for giving a correct idea of the surface of that luminary, and the changes which have taken place in it, in its atmosphere and in its mountains; by Dr. F. F. Schroeter, grand bailiff of Lilienthal, near Bremen. Part ii. 1802. pp. 565. 4to. with 32 plates. . Göttingen.

The first part appeared in 1791: we published two extracts from it, which show with what patience and minuteness this able astronomer has examined the surface of the moon.

These new observations were made with the same care for twelve years, and with optical instruments of the greatest power, (two telescopes, one of 13 and the other of 27 feet.) They entitle him to the gratitude of astronomers, as they will serve them as a base and term of comparison in their future researches respecting the changes that may take place at the surface of the moon. He has observed mountains which rise to the height of 4000 toises, and others to the height of 2400. M. Schroeter has also determined, that the part of the lunar atmosphere which is sufficiently dense to produce the crepuscula, is 300 toises in height.

The author has also seen objects which he did not see in the course of his preceding observations, and which on the first view might be ascribed to changes that have taken place at the surface of the moon: but he remarks, with the reserve of an able observer, that the particular state of the lunar atmosphere may have concealed these objects at the time of his first observations.

Dr. Henzenberg, of Hamburg, between the months of July and October 1802, made thirty-one experiments on the fall of bodies from the steeple of St. Michael, which is 235 Paris feet in height: and found that heavy bodies do not fall vertically; there are four lines of declination towards the east, and 1.5 line towards the south.

M. Guglielmini, of Bologna, found a little more. But all these experiments tend to prove the rotation of the earth.

[To be continued.]

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XXXII. No-

XXXII. *Notices respecting New Books.*

*Elements of Chemistry.* By JOSEPH FRANCIS JACQUIN.  
Translated from the German. 8vo.

A SECOND edition of this work has made its appearance; a proof of the favourable reception it has experienced from the public. We have no hesitation in saying that it is a useful work, and ought to make a part of every chemical library. The language possesses that precision which constitutes the chief merit of a translation, especially from a scientific or practical work. It is by Mr. Stutzer.

*A System of Chemistry.* By THOMAS THOMSON, M.D.  
4 Vols. 8vo. 1802.

The task of composing elementary treatises, as the author justly observes, has in this country been usually left to men of inferior endowments, as a piece of drudgery below the dignity of a philosopher; while in France, excellent systematic treatises have appeared on almost every part of science by men of the first abilities. Though we can rank several distinguished names among our authors of elementary works, it is certain that “we have more frequently satisfied ourselves with translating the systematic works of foreigners, even when the discoveries of our own philosophers had furnished a considerable portion of the materials of these works. The consequence has been, what was naturally to be expected, the labours of our philosophers have been frequently overlooked, and their discoveries claimed by others to whom they did not belong; while these claims, constantly inculcated in all the elementary treatises of chemistry, have been received as first principles by the greater number of readers. It is incontrovertible, that, for the rapid progress which chemistry has lately made, the science is deeply indebted to the philosophers of this country. Much, indeed, has been done by the illustrious body of French chemists; but these gentlemen, not satisfied with a part, have laid claim to the whole.”

The author states his work to have for its object the giving a full detail of the vast number of facts which constitute chemistry, blended with the history of their gradual development, and of the theories which have been founded on them, and accompanied with exact references to the original works in which the different discoveries have been registered. It is but justice to say, that Dr. Thomson has executed the task he has undertaken in a manner creditable to himself. His work presents

presents a more complete and accurate collection of facts than any previous publication of the kind. The arrangement is judicious, the quotations are candid, the reasoning satisfactory and perspicuous. We have no hesitation in recommending it as a most valuable performance.

*The Chemical Pocket-Book, or Memoranda Chemica; arranged in a Compendium of Chemistry.* By JAMES PARKINSON. 12mo. 1803.

It gives us pleasure to announce that a third edition of this valuable little work has made its appearance, embracing all the new facts in chemistry that have been observed since the publication of the former editions. A considerable degree of care seems to have been bestowed upon it, and several new and useful tables have been added in different parts.

*A System of Theoretical and Practical Chemistry, &c.* By FREDERICK ACCUM, Teacher of Practical Chemistry, Pharmacy, and Mineralogy; and Chemical Operator in the Royal Institution of Great Britain. 2 Vols. 8vo. with Plates. 1803.

This work, just published, will prove highly acceptable to those who wish to attain a practical acquaintance with the science of which it treats. The author exhibits throughout the work a great degree of chemical knowledge; and his method of communicating knowledge to others is engaging. With the practical part of the various processes in chemistry he seems to be well acquainted; and the accuracy and precision with which he describes them, will render his work valuable not only to tyros, but to proficients in the science. Many of the experiments, some of which are calculated to afford both instruction and amusement, are quite new; and even in detailing experiments which are not new, particulars not to be met with elsewhere are often given, which will be useful to the practical chemist.

### XXXIII. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY OF LONDON.

SINCE our last report several meetings have been employed in reading a paper on tanning, by Mr. Davy, professor of chemistry in the Royal Institution of Great Britain. In this paper the author not only describes the processes usual in the art of tanning, but enters into a chemical investigation of the

nature of the agents employed in the process, and details a number of ingenious experiments undertaken expressly for the purpose of ascertaining the mode of their operation. The results are interesting, and cannot fail to be useful to those engaged in the manufacture of leather.

#### ROYAL ACADEMY OF SCIENCES AT BERLIN.

On the 18th of January the academy held a public sitting to celebrate the anniversary of its establishment. It was opened by the director Merian with an oration; after which M. de Verdy du Vernois read the sketch of a plan for reviving the antient tournaments, and showed the utility of again introducing this antient amusement, formerly a school for dexterity and intrepidity, and which might be attended with great advantage to the cavalry service, and to all troops that fight on horseback.

M. Erman then read a paper containing particulars respecting the reign of the elector Frederic William the Great, extracted from a manuscript diary of marshal Dieterich Sigismund von Buch.

M. Klaproth next read a dissertation on stones and iron masses, and showed that the fall of such bodies from the heavens is a phenomenon totally different from all the other phenomena of nature; and that it needs therefore excite no wonder that doubts have been entertained respecting the truth of such facts, but that hitherto they are not only supported by experience, but by chemical analyses of the fallen bodies. As a contribution towards this subject, M. Klaproth gave an account of experiments, made by himself; with some of these productions which had fallen at different times and in different countries, particularly a specimen of that which fell near Siena on the 16th of January 1794; and a fragment of the mass of iron, weighing 71 pounds, which fell near Agram on the 26th of May 1751, comparing them with the analyses lately published by Mr. Howard of the stones which fell at Benares in the East Indies on the 19th of December 1798, and on the 13th of December 1795 in Yorkshire; and adding a short history of these and some other bodies that have fallen from the clouds. They consist either entirely of cellular masses of iron, or of stony substances mixed with grains of iron. In all of them the iron is of the same nature: it is ductile, exceedingly tough, appears white when filed, and almost always contains nickel. The stones are covered on the outside with a black crust, are bright gray in the inside with black spots, and interspersed, besides the ferruginous particles, with grains of pyrites. The principal part of the

mass consists of oxide of iron; magnesia; and flint. The opinion of Dr. Chladni, that these meteoric productions are fragments of fire balls which have burst, may be considered as fully established. Real native iron from the mines of Kamsdörff is distinguished from the meteoric iron by not containing any nickel, and by containing a mixture of copper-lead.

## FRENCH NATIONAL INSTITUTE.

Account of the labours of the Mathematical and Physical Class during the first quarter of the year 11.

MATHEMATICAL PART, read by C. Lacroix.

## ASTRONOMY.

*Observations on the Transit of Mercury over the Sun's Disc,  
November 9, 1802.*

If the theory of attraction, aided by the powerful means of analysis, has enabled astronomers to give, almost at once, to the tables of the planets, and particularly to those of the moon and sun, a degree of exactness far superior to that to which they could have been carried by the efforts of observers alone during a long series of ages, time has not, on that account, lost its right to the perfection of these results deduced from the measurement of its duration. Astronomers therefore, always attentive to the spectacle of the heavens, never suffer to escape but with regret opportunities of observing the simplest and most frequent phenomena, because they know that, however precise they may be for the moment at which they are assigned, astronomical determinations have continual need of being rectified: with much more reason, therefore, they are anxious to take advantage of those circumstances where the position of the planets is less affected by the combination of their own motion and that of the earth. Such in regard to Mercury are his transits over the disc of the sun.

The first of these phenomena preserved in the annals of astronomy was observed in 1631, at the college of France, by Gassendi, one of the most illustrious professors of that celebrated school. Since that time great attention has been paid to this phenomenon; and the observations of these transits, which follow each other pretty rapidly, have been multiplied. C. Lalande has employed himself in collecting and examining them with a care and assiduity for which he has been rewarded by the perfection he has thence been able to give to his tables of Mercury. After having successively corrected each element, he waited for a confirmation of them by the

transit announced for the 9th of November 1802, and hoped he should then be able to be convinced that his tables, one of the most important results of his long labours, had reached the utmost perfection to which they could attain in the present state of the science. His wishes have been fully gratified by the observations we are about to detail.

The moments of interior and exterior contact of the two bodies on the egress of Mercury were determined as follows:

By	{	Lalande	-	-	0 <sup>h</sup> 6' 29 <sup>''</sup>	7 <sup>l</sup> 56 <sup>''</sup>
		Messier	-	-	0 6 49	8 20
		Lalande jun.	-	-	0 6 44	8 19
		Bouvard	-	-	0 6 54	8 19
		Mechain	-	-	0 6 45	8 30
		Burckhardt	-	-	0 6 45	8 20

C. Lalande takes as the mean terms of the two contacts 0<sup>h</sup> 6' 49<sup>''</sup>, and 0<sup>h</sup> 8' 19<sup>''</sup>;

From which he deduces the egress of Mercury at

0<sup>h</sup> 7' 34<sup>''</sup>;

The apparent distance of the centres at that moment,

16' 8.3<sup>''</sup>;

And the same distance reduced to the centre of the earth,

16' 10.0<sup>''</sup>.

By then comparing three differences of declination selected from twenty-five, which were measured by C. Messier during the transit, he deduces from them the shortest distance of the centres, namely 65<sup>''</sup>: and combining it with the distance of the centres at the moment of the egress, he is enabled to find the difference in longitude and latitude of the two bodies, and the time elapsed between the middle of the transit and the egress. We shall not follow the detail of his process any further, as our astronomical readers may easily supply what is here omitted; and to others it would be superfluous. We only state the results which C. Lalande obtained by this first method.

He found the true conjunction at

9<sup>h</sup> 2' 40<sup>''</sup> mean time;

The longitude, reckoned from the mean equinox for the moment,

7<sup>s</sup> 16° 17' 9<sup>''</sup>;

The geocentric latitude,

53<sup>''</sup> North;

The heliocentric latitude,

1' 54<sup>''</sup>;

The correction of the tables: — 13<sup>''</sup> in longitude.

C. Lalande

C. Lalande re-commenced his calculations; substituting for the differences of declinations the observation of Mercury's passage of the meridian, made the same day by his nephew and C. Burckhardt; and by these means found  $2''$  less in longitude, and  $6''$  less for the latitude, at the moment of the egress.

He determines the geocentric latitude at the time of conjunction to be  $56.5''$ , which gives the place of the node  $1^{\circ} 16' 0'' 37''$ , further advanced by  $1' 46''$  than in the tables; and as C. Delambre had found by the passage of 1799 that this element ought to be increased by  $1'$ , C. Lalande adopts a mean correction of  $+ 1' 23''$ , which makes the place of the node in 1801 to be

$$1^{\circ} 15^{\circ} 58' 54''.$$

By comparing this determination with that which he deduced from the transit of 1677, he found for the annual motion of the node  $43.98''$  instead of  $43.3''$ , which he had before.

To ascertain whether the position which he gave to the aphelion of Mercury was exact, C. Lalande examined again the transit observed by C. Delambre in 1799, taking into account the perturbations calculated by M. Oriani, and the error in the tables of the sun, and he found the same error of  $13''$  as in that of this year, though the anomalies were very different. He consequently thought that this error ought to be ascribed only to the epoch of the mean motions: but after a communication from Dr. Maskelyne in regard to a correction of  $4''$  to be made in the right ascensions, which he assigned to the positions of the principal stars employed for determining the places of the sun, the error in the tables of Mercury would be reduced to  $9''$ , and the epoch of the mean motions of this planet would consequently be for 1801,

$$5^{\circ} 11^{\circ} 53' 32''.$$

By distributing this error to the secular motion it would be reduced  $6''$ , and would become

$$2^{\circ} 14^{\circ} 4' 4''.$$

In a word, the transit of November 9, this year, has fully confirmed the result which C. Delambre deduced from that of 1799 in regard to the diameter of Mercury. C. Delambre had obtained for this diameter as seen from the sun  $6.3''$ ; and C. Lalande found  $6.2''$ , while in his Astronomy it was  $6.9''$ .

C. Messier communicated to the class a particular account of his observation of the transit of Mercury, during which he successively determined twenty-five positions of that planet,

comparing it either with the sun's limb, or to a very considerable spot which appeared on that body, and of which the diameter was  $25'$  of a degree. He determines the moment of the egress of Mercury's centre at  $7^h 6^{m}$  true time; and this planet's passage of the meridian  $55^{m}$  before that of the sun's centre.

C. Messier's memoir contains a detailed view of the determinations he obtained, and is accompanied with a drawing which represents the apparent route of Mercury on the sun. It exhibits a luminous ring, with which Mercury seems to be surrounded. This circumstance, peculiar to the observation of C. Messier, was remarked by him in the transit of 1799; it was observed also at Montpellier, in 1736, by Plantade; and at Upsal, in 1736, by M. Prosperin. This ring exhibited a very faint light, and of a colour different from that of the sun. Its diameter towards the end of the transit was  $1' 19''$ , and that of Mercury  $17''$ .

#### EXPERIMENTAL PHILOSOPHY.

##### *On the Method of magnetizing to Saturation.*

To apply with constancy to the same objects, and to vary their combinations in every manner possible, are the conditions on which nature consents to disclose her secrets to those who interrogate her. It was by fulfilling them with an assiduity and sagacity worthy of success that C. Coulomb attained to his interesting discoveries in electricity and magnetism, and by imposing on himself the task of illustrating every part of these important branches of philosophy. *Magnetizing*, the perfecting of which is of so much importance to navigators, could not escape his attention. Very vague notions had hitherto been acquired in regard to the intensity of the magnetic force which steel is capable of acquiring in its different states; and it was measured in general by the weight of the piece magnetized. But it is of the greatest importance here to know the energy with which needles tend to return to their direction after they have been removed from it; and this C. Coulomb measures directly, by determining, according to Borda's views, and by means of very nice suspension, how many oscillations these needles perform in a given time.

After comparing the methods of magnetizing proposed by Duhamel, Knight, and Æpinus, to give a needle all the directing force it is capable of receiving, the author decides in favour of that of Æpinus. He found that it is most advantageous to make the needles long and broad, but not thick.

thick. C. Jekel has established a manufactory in order to make an useful application of these results for the use of the navy.

PHYSICAL PART, read by Lacroix.

*On the Stones supposed to have fallen from the Clouds.*

The attention of philosophers has been again directed to the singular stones the origin of which is unknown, and which are supposed to have fallen from the clouds. The French chemists were desirous of proving by experiments the identity of these stones, and of ascertaining their component parts, already indicated by Mr. Howard.

C. Vauquelin procured some specimens of the stones, analysed by Mr. Howard, found at Benares in the East Indies, in Yorkshire, at Siena in Italy, and at Bologna: to which he added some of those which fell in France in 1789; at Barbotan near Roqueton, and Creon in the parish of Juliac, in 1790. He remarked, as Mr. Howard did, that these stones have such a perfect resemblance to each other that it is almost impossible to distinguish them. He was convinced by various analyses that they all contain the same principles, namely, silica, manganese, iron, nickel, and sulphur. These results, analogous to those obtained by Mr. Howard, and the work in which Dr. Chladni, known by his ingenious experiments on vibrating surfaces, has collected all the accounts published respecting the fall of these stones, concur to render it probable that their origin is foreign to our globe; for hitherto none of a similar kind have been found in the interior parts of it.

The reading of this interesting memoir gave rise to a discussion the results of which must be here mentioned, as they afford new motives to induce philosophers to examine and appreciate the different testimonies, in consequence of which the stones in question have been supposed to have fallen from the clouds. When a phenomenon is announced, if we were able to ascertain, by a complete enumeration of the different physical agents, that none of them is capable of producing it, the impossibility of the phenomenon would be the evident result, and consequently the falsity of the account.

But, on the other hand, when we find a cause which establishes the probability of it, if sound logic forbids us to ascribe it exclusively to this cause, it commands us at the same time to substitute doubt for complete negation, and to employ every means possible of confirming the fact, because it is not repugnant to the general laws of nature.

Chemists

Chemists at present would be much embarrassed to find in the atmosphere the component principles which have been discovered by analysis in the stones given to them as having fallen from the clouds; and on this account they would naturally reject such events as absurd. But C. Laplace has mentioned an explanation, which he announces not as the only one which could be given of them, and not to prove their existence, but to show that we ought not to reject them as absurd, and to suspend our opinion until time has procured some further illustration.

It is shown, by a very simple calculation, that a body projected from the moon would require only a velocity nearly quintuple that of the bullet of a 24-pounder discharged with a quantity of gunpowder equal to half its weight, to proceed to a distance from that satellite where the intensity of its attraction would be the same as that of the earth. The body, when it had passed this point, being in the sphere of the activity of our globe, would necessarily fall to its surface. The appearance of very considerable volcanoes observed on the surface of the moon does not render such a conjecture improbable; but, independently of eruptions, which may be more or less uncommon, it will not often happen that the direction of the projection will be that which the combined motions of the earth and moon require in order that a projectile thrown from the latter body may meet with the former.

The atmosphere of the moon, which is doubted by many astronomers, is so rare and of so little extent that it could oppose only a very feeble resistance to those bodies which might move in it. The case is not the same with the terrestrial atmosphere: it reduces almost to a tenth of its extent the largest range of a piece of ordnance; and the resistance it opposes to rapid motion is such, that to make a body projected from Vesuvius reach France, it would be necessary that it should have a projection far greater than that which would carry a lunar body to the limits of the sphere of the activity of that satellite.

It is therefore not possible to suppose that stones found at a distance from terrestrial volcanoes are the product of the eruptions of these mountains: and mineralogy also opposes this explanation; for none of the volcanic productions known are analogous to those supposed to have fallen from the clouds.

## METEOROLOGY.

*On the Variations of the State of the Heavens in the Mean Latitudes between the Equator and the Pole, and the essential Circumstances by which they are accompanied.*

The influence of meteors on the results of agriculture has rendered it of importance at all times that the law of the succession of these phænomena should be known; and the great advantages which would arise to mankind from the possibility of foretelling the nature of the seasons, sufficiently justifies all the fruitless attempts hitherto made to accomplish that end.

Being much struck with these advantages, C. Lamarck was discouraged by the bad success of those who had preceded him in this career; and from continuing to pursue it with ardour, after publishing in different works the causes which he assigns to the prevailing meteorological constitutions, he has given in a late memoir an examination of the variations of the state of the heavens, that is to say, of the atmosphere: he refers this state,

1st, To the influence of the sun's light:

2d, To that of the winds.

But he is of opinion that in the temperate zones the influence of the winds on the temperature of the strata of the atmosphere is much stronger than that of the light of the sun; and that there thence result in the density and heat of these strata, which naturally become rarer and colder the higher they go, inversions to which he ascribes the formation of the clouds.

The discordance of the effects which the same wind produces at different times seemed to oppose to the establishment of a theory of these effects an insurmountable obstacle: but from numerous observations C. Lamarck is of opinion, that besides the direction and nature of the winds alone, attention ought to be paid also to the height at which they blow in the atmosphere.

Of the facts collected by the author, and which he considers as sufficiently proved, we shall mention the following: "When a *north-west* and a *south-east* wind prevail simultaneously in two different strata of the atmosphere, if the *south-east* be the lower one, we may expect to see the weather become clear; but if this wind be the higher, the contrary will take place."

XXXIV. *Intelligence and Miscellaneous Articles.*

## ASTRONOMY.

WE are indebted to a very accurate and much valued correspondent for the following tables, which cannot but prove highly acceptable to such of our readers as are desirous of knowing in what part of the heavens the two new planets may be expected to be found. We believe no Ephemeris has yet been published containing their right ascensions and declinations. Baron Von Zach has published one in which their geocentric latitudes and longitudes are given; but this work being written in German is in the hands of but few people in this country.

Geocentric Motion of Ceres Ferdinanda.			Geocentric Motion of Pallas.		
1803.	Right Ascension.	Declin. South.	Right Ascension.	Declin. North.	
April 3	18 <sup>h</sup> 54 <sup>m</sup> 47 <sup>s</sup>	23° 6'	18 <sup>h</sup> 47 <sup>m</sup> 0 <sup>s</sup>	13° 33'	
6	18 57 21	23 10	18 48 36	14 3	
9	18 59 43	23 15	18 50 4	14 34	
12	19 1 4	23 19	18 51 20	15 5	
15	19 3 56	23 24	18 52 28	15 36	
18	19 5 46	23 30	18 53 24	16 7	
21	19 7 23	23 36	18 54 8	16 38	
24	19 8 49	23 43	18 54 44	17 9	
27	19 10 3	23 50	18 55 4	17 39	
30	19 11 5	23 58	18 55 16	18 9	

The above table, containing the geocentric motion of the two new planets, Ceres Ferdinanda and Pallas, for the ensuing month, is selected from an Ephemeris calculated by Mr. Gauss, of Germany, of whose abilities as a calculator the world has already been fully convinced. Mr. Gauss does not expect that the observed places of these planets when they shall be again found will agree very correctly with his Ephemeris, as there has not yet been a sufficient number of observations made to settle the elements of their motions with such a degree of precision; but he hopes his calculations will render them very easy to be found: those gentlemen, therefore, who are provided with equatorials, or instruments for taking angles out of the meridian, will no doubt

doubt be soon able to find both, or at least Pallas, whose situation is now become pretty favourable, being at a sufficient distance from the sun to be seen in the morning when the weather is clear. It does not appear likely that Ceres Ferdinandea, from her great south declination, will be seen here till the latter end of April, or perhaps the beginning of May; but those astronomers who are situated in more southern latitudes will view her under more favourable circumstances, and may perhaps be able to find her sooner.

#### HUMBOLDT'S TRAVELS.

Where Alexander von Humboldt may be at present is not known even to his relations; but all the observations which he collected up to the beginning of March 1801 have lately arrived safe in Europe. He entrusted this valuable deposit, which consists of four bound volumes and three manuscript bundles, a roll of plans, and a number of drawings of plants, to one of his friends at the Havannah, where he then was, to be sent to Spain by a proper opportunity. They arrived in the beginning of September 1801, and have since been transmitted to Berlin.

#### FOSSIL BONES.

Two horns, with the head and several bones of an unknown animal of a monstrous size, have lately been found in the Mologí circle, of the government of Jaroslavi, on the banks of the Mologa, on an estate belonging to count Alexei Iwanfwitch Puschkin. The length of the head is two ells Leipzig measure \*, and the breadth on the forehead one ell eight inches, (two feet five inches.) The horns are like those of an ox, are about four ells in length (six feet eleven inches), and in the thickest part are about an ell in circumference (one foot nine inches). Gmelin, as is well known, says that, besides bones of the mammoth, (different however from those of the American mammoth,) heads with horns of an extraordinary size are sometimes found in Siberia. He himself carried with him to Peterburgh a head of this kind, which in all probability is still preserved in the cabinet of curiosities belonging to the Academy of Sciences. It will be interesting to natural history to compare this head with the one lately found, in order to ascertain whether they belong to the same species of animal. At any rate, these heads seem to afford a new proof, in opposition to the opinion of Daubenton, Buffon, and other naturalists, that there existed formerly species of animals now become extinct.

\* About one yard six inches.

## ANOTHER NEW METAL.

Professor Tromsdorff, of Erfurt, has published the following notice:—"One of my mineralogical friends, whose name I shall make known at a proper time, found lately a particular substance which he transmitted to me for examination. Though the quantity was small, I am however convinced that it is a *new metal* combined with sulphur. The characteristic marks of this metal are: It belongs to the volatile metals: with sulphur it forms a mass which melts like wax, and which crystallizes; with sulphureous acid it gives a reddish solution; with nitrous acid and nitro-muriatic acid it gives a yellowish solution; muriatic acid has no action on it warm: from acid solutions it is precipitated green by the prussiate of potash; of a steel gray by tincture of galls; chamois yellow by *hydrothion-ammonia* \*; by carbonate of potash it is precipitated as a white oxide; it is not precipitated from its solution by caustic ammonia, and it is probable that it forms with it a double salt. I shall observe in the last place, that this new substance is found in Germany. A further account of it will be published in my journal."

Erfurt,  
Jan. 1803.

\* By this term we conceive the author to mean a solution of carbonate of ammonia, in contradistinction to pure (caustic) ammonia, which he afterwards mentions.

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XXXV. Letter from Dr. BARTON to Professor ZIMMERMANN, on the fascinating Faculty which has been ascribed to the Rattlesnake, and other American Serpents \*.

DEAR SIR,

Philadelphia, Feb. 12, 1800.

I HAVE never yet received the translation which you have made of my "Memoir concerning the fascinating Faculty which has been ascribed to the Rattlesnake, and other American Serpents." I feel flattered by your kind notice of that little production, one of my first essays in natural history, and one to which, I confess, I devoted a good deal of attention. By transferring my sentiments on the subject which I have touched into the language of your country, you have, no doubt, contributed to draw the attention of the learned to the discussion of the question, whether serpents are endued with the power of fascinating other animals.

I did not, before this day, know that our learned friend professor Blumenbach, of Göttingen, had published some Remarks on the Memoir, in Voigt's *Magazin für den neuesten Zustand der Naturkunde*, part ii. I have not seen the magazine, but have met with a translation of the professor's paper in the *Philosophical Magazine* †, published in London by Mr. Alexander Tilloch. In this letter I am going to trouble you with some further remarks, which you are at liberty to make use of in any way you may think proper.

I shall not pursue the professor in the precise order of his remarks. I shall first take notice of his defence of that passage in his *Manual of Natural History* ‡, which I particularly examined in my memoir, and which he seems to think I have criticised with somewhat of severity. The following are Mr. Blumenbach's words, as I have translated them in the memoir:—"That squirrels, small birds, &c. voluntarily fall from trees into the jaws of the rattlesnake lying under them, is certainly founded in facts: nor is this much to be wondered at, as similar phænomena have been observed in other species of serpents, and even in toads, hawks, and in cats, all of which, to appearance, can, under particular circumstances, entice other small animals by mere steadfast looks. Here the rattles of this snake (the rattlesnake) are of peculiar service; for their hissing noise causes the squirrels,

\* Communicated by Dr. Barton.

† For December 1798.

‡ *Handbuch der Naturgeschichte*.

whether impelled by a kind of curiosity, misunderstanding, or dreadful fear, to follow it, as it would seem, of their own accord. At least," continues Mr. Blumenbach, "I know, from well-informed eye-witnesses, that it is one of the common practices among the younger savages to hide themselves in the woods, and, by counterfeiting the hissing of the rattlesnake, to allure and catch the squirrels".

On this passage I made some remarks, which were brought together under three distinct heads. I observed, 1st, That "the faculty of fascinating is by no means peculiar to the rattlesnake, but is attributed as extensively to the black snake, and other serpents, which are not furnished with the *crepitaculum*, or set of bells." 2dly, That "some persons, who have seen the rattlesnake in the supposed act of charming, assure me that the reptile did not shake its rattles, but kept them still;" and lastly, I was inclined to think that there was no solid foundation for the story upon which, in part at least, Mr. Blumenbach has founded his theory.

It is certainly the prevailing opinion in this country, that the black snake and other serpents, as well as the rattlesnake, are endowed with the faculty of fascinating or charming other animals. It is well known that none of the serpents except the different species of *crotalus* are furnished with the *crepitaculum caudæ*, or rattle, and no attempt has been made to point out the difference (if there be a difference) in the modes of fascinating employed by these several serpents, with the exception of the remark made by a few of the Indians and Whites, that the rattlesnake charms with its rattle †. But we shall afterwards see that this is by no means the general opinion among the Indians or Whites.

Since the printing of my memoir, I have been assured by a very intelligent person ‡, who, living in a part of the country which abounds in rattlesnakes, has had many opportunities of attending to the manners of these serpents when watching for their prey, that at such time the rattlesnake does not move his rattle, but lies still. This is a confirmation of the fact mentioned in my memoir. "It is very probable," however, as M. Blumenbach observes, "that the case here may

\* See a Memoir, &c. pages 46 and 47.

† See a Memoir, &c. pages 14 and 15.

‡ Mr. Samuel Preston. "Dr. Mead (says Mr. Preston) supposes the rattle on their tail to be useful for that purpose (viz. charming); but he is much mistaken, as I have actually seen them engaged in the process a number of times. They do not make any noise with their rattle; they lie perfectly still, with an open mouth and sparkling eyes."—Letter to me, dated Stockport, August 7, 1793.

be altered by circumstances;" that is, that the reptile whilst endeavouring to obtain its prey sometimes shakes its rattle and sometimes keeps it still. I am now, however, fully persuaded that the latter is the general case; and the more so, because it seems to be ascertained that the rattlesnake seldom shakes his rattle unless when he is irritated.

If then this serpent does not always, nor even generally, move his rattle at the time he is watching for his prey, it is, I think, somewhat unphilosophical to ascribe so much to the "hissing noise" of the instrument as Mr. Blumenbach has done. "This lazy animal, when lying on the ground, might certainly," says the professor, "employ that singular organ for enticing animals, as well as the cerastes employs its horns for the same purpose, at least according to common report."

I will readily allow, that if the cerastes of Egypt is capable of charming by means of the horns upon his head, the rattlesnake might, without much stretch of the imagination, be supposed capable of charming by means of the horny bells upon his tail. But here Mr. Blumenbach attempts the solution of one difficulty by having recourse to another difficulty; or, rather, he seems disposed to cut the knot of fascination, as it respects the rattlesnake, by means of a story which has never been proved to be a fact, and which, I cannot but think, is one of the many improbable tales in natural history. I know it is sanctioned by Pliny\* and Solinus. But, alas! how many hundred anile stories has Pliny told! and who does not know that Solinus is often the servile copier of Pliny?

In the discussion of curious questions like the present, I can feel pleasure even in furnishing my opponent with implements with which to defend himself. With this disposition of mind I will here mention a supposed fact, which is a good deal similar to that related of the cerastes. I do not doubt that it is as worthy of belief. There inhabits the lakes and rivers of many parts of North America a very curious species of esox or pike, which is commonly known by the name of the gar fish or bill fish. The upper jaw is lengthened out into a long bony rostrum, or protuberance, which has given it the name of *bill fish*. Although this fish is known to live almost entirely upon other fish, to which he proves very destructive, we are told that the gar hides himself in the reeds in such a manner, that nothing but the curious rostrum, which he thrusts out of the water in a perpendicular position,

\* "Cerastris corpore eminere cornicula sæpe quadrigemina: quorum motu, reliquo corpore occultato, sollicitent ad se aves." C. Plinii Secundi Naturalis Historiæ Lib. viii. cap. 23.

can be seen. Different kinds of birds, which come to rest themselves upon the reeds, mistaking the fish's bill for a reed, or dry piece of wood, perch upon it also. He then opens his mouth, and generally makes an immediate prey of the misguided bird.

This story is related by a very respectable writer, father Charlevoix, who adds a circumstance which I must not omit to mention, especially as it would seem to show that there is some hidden (we will call it a *fascinating*) virtue in the gar's bill. The Indians say, that the sharp teeth which are distributed along the edges of this instrument are a "sovereign remedy against the head-ach, and that pricking with one of these teeth where the pain is sharpest, takes it away instantly \*."

But to be serious: As most, if not all, animals are furnished with an organ of hearing, so it is not improbable that different sounds, particularly the sounds of musical instruments, have something charming or attractive to certain species of animals. Passing by the stories that are told of the *trichechus manatus*, or sea-cow, the common mouse, and some other animals; I will here mention a fact related by Dr. Odier on a very respectable authority, and I relate it the more confidently because it has been confirmed to me by several persons of credit. The iguana † of the West Indies is said to be so fond of music, that at the sound of an instrument this ugly looking lizard becomes almost motionless, and is easily taken by a noose ‡.

But I must return to Mr. Blumenbach. "I know," says

\* A Voyage to North America, vol. i. pages 117 and 118. English translation.

† *Lacerta iguana* of Linnæus.

‡ See *Epistola Physiologica, Inauguralis, de Elementariis Musicæ Sensationibus*, Nota 32, *Edinburgi* 1770. It were much to be wished that some ingenious man would favour us with a memoir concerning the influence of music upon different animals. Some interesting materials for such a work are to be found scattered through a number of writers both ancient and modern. But new materials might readily be collected, since it would not be a difficult task to make experiments. Much curious physiological knowledge would result from such an inquiry: and I greatly mistake if the inquiry would not somewhat tend to diminish the quantity of our prejudices against animals that are unquestionably innocent. Should it have this effect, how great would be the gain to a benevolent mind! Nurtured among prejudices of different kinds, we are at once miserable and unjust. I must confess that I have dropped some of my prejudices against the amphibia (See a Memoir, &c. p. 45, note) since I have learned that the iguana is pleased with the music of the West Indians; and that a little garden lizard listened, "with a breathless attention," to the sound of a lady's piano-forte.—For a very interesting instance of this kind, see the *Analytical Review* for January 1789.

he,

he, "from well-informed eye-witnesses, that it is one of the common practices among the younger savages to hide themselves in the woods, and, by counterfeiting the hissing of the rattlesnake, to allure and catch the squirrels." After quoting this passage, I observed in my memoir, that "I have inquired of Indians, and of persons who have resided for a considerable time among the Indians, and they appear to be as ignorant of the circumstance as I am myself." I continued, "I am inclined to think that Mr. Blumenbach has been imposed upon," or, perhaps, that a circumstance which I have related may have given rise to the story\*. Mr. Blumenbach has since informed me by letter, and now informs the public in his Remarks, that he received his "information from major Gardner, who, with his family, resided many years in East Florida. He is," says the professor, "a very intelligent naturalist, an accurate observer, and certainly would be very far from imposing upon me."

I knew not, when I printed my memoir, from whom Mr. Blumenbach received his information. It was not, however, unnatural for me to suppose, that he had been "imposed upon," because I well know that some of the most respectable naturalists and historians of Europe have often been most grossly deceived by travellers who have visited this country: hence the many tales and scandalous stories which crowd and deform some of the works of your most celebrated writers. Such tales and stories are the following:—That the Indians have no beards; that they have very small appetites; that they are greatly addicted to the "antiphyfical vice;" that *none* of the tribes knew any thing of the use of salt before the Europeans came among them; that they cannot carry their arithmetic beyond the numeral three; &c. &c. &c.

I have lately made further inquiries of the Indians concerning the stratagem which, Mr. Blumenbach says, the younger savages employ to allure to them squirrels. I can learn nothing concerning it. I am still disposed to think that there is but a slender foundation for the story. I am certain that it is not a *common* practice among the Indians. Persons who have resided for many years among our northern and western Indians have never heard of it: neither have intelligent traders and interpreters from the very country in which major Gardner resided. Anxious to ascertain the truth, I shall extend my inquiries, and, should I learn that the Indians do actually employ the stratagem, I shall endeavour to be the first to inform Mr. Blumenbach of the success of my research.

\* See a Memoir. &c. pages 48 and 49.

I shall conclude this part of my letter with two observations; viz. 1st, If it be a fact, as I have asserted, that the rattlesnake, while watching for his prey, seldom moves his rattle, the story related by major Gardner must, independently on any other facts, appear highly improbable. 2dly, Even admitting the fact, that the rattlesnake does shake his rattle while employed in the supposed act of charming, I think the greater number of the favourers of the existence of a fascinating quality in this serpent ought, upon a careful consideration of the subject, to give up Mr. Blumenbach's explanation of the business. They tell you that the bird or squirrel is often seen precipitating itself from the top of a lofty tree into the jaws of the serpent lying at the bottom. Now, is it likely that the noise of the rattle can be distinctly heard at the distance of sixty, eighty, or a hundred feet, in a forest where all else is not silence; in a forest where rooks, and ravens, and jays, wood-peckers, and many other species of birds, utter their various cries or notes, which are mixed, and often confounded, with the noise of tree-frogs, locusts, and a hundred other animals? I think, sir, you will admit with me, that it is very improbable, if not impossible, that the rattle could affect the bird at such a distance; and I am disposed to believe, that, after reading the preceding observations, you will not think Mr. Blumenbach's system is so capable of being maintained as that ingenious gentleman seems to suppose it is.

Mr. Blumenbach, in taking notice of my theory of accounting for the supposed fascinating power of serpents, informs us, that with this method of explaining the phenomenon he has been acquainted since 1785, from an essay by professor Michaëlis, in the Göttingen Magazine for January of that year. In justice to Dr. Michaëlis, I shall here quote his words, as they are given by Mr. Blumenbach:—"Others believe that it is owing merely to the care of the old ones for their young, which throw themselves between the latter and their enemies, and by these means become a prey to them. One of my friends, Mr. David Colden, at Flushing, an amateur of natural history, and son of governor Colden, whose service to science is so well known, assured me that he had several times seen birds fascinated by snakes, but always found the nest of the bird either with eggs or young ones in the neighbourhood; which made the spectators give up the idea of fascination. But," Mr. Michaëlis adds, "I know some instances where no nest could be in the neighbourhood, and where, though the snake was at first at a great distance from the bird, it nevertheless fell towards it."

From

From this quotation it appears that I was not the first person who endeavoured to explain the supposed fascinating power somewhat in the manner I have done. I have not asserted that I was. But I certainly neither did nor could borrow the explanation from Mr. Michaëlis, whose "valuable essay," as Mr. Blumenbach calls it, I have never yet seen. My theory, which every day's inquiry serves to strengthen, was the result of a great deal of attention to the subject: and I have enjoyed as many opportunities of investigating the truth as Mr. Michaëlis did. With respect to that ingenious gentleman, I should have allowed him more merit had he adopted Mr. Colden's explanation; and I can allow him very little for rejecting it, merely because, in some instances, no nest could be found in the neighbourhood, and because, "though the snake was at first at a great distance from the bird, it nevertheless fell towards it." It surely does not follow, because no nest could be found, that none existed. The schoolboy well knows the difficulty of discovering the nests of many species of birds; and the naturalist, who ought to be acquainted with the arts employed by these animals to conceal, from man and other enemies, their nests, should make still greater allowance for the difficulty of discovering these nests. As to Mr. Michaëlis's other assertion, that, "though the snake was at first at a great distance from the bird, it nevertheless fell towards it," I will not positively deny it, until I learn whether that gentleman has himself witnessed any thing of the kind. But, in the meanwhile, I must say, that I have no reasons to think that I have been precipitate in advancing what I have advanced on this subject in my memoir.

I do not perceive that Mr. Blumenbach has made any other attempt to controvert my theory, except in so far as he has mentioned Mr. Michaëlis's two observations just noticed. On the contrary, in the new edition of his Manual, the professor has quoted my memoir, and expunged the story about the ringing of the serpent's tail \*. Of Mr. Michaëlis's essay he makes no mention. And here, before I adduce any additional facts in support of my explanation, I cannot forbear to observe, that I do not think Mr. Blumenbach has done justice to this part of my memoir. In particular, the fine fact communicated to me by the late Mr. Rittenhouse, of which he has made no mention, is worth a whole volume of speculations on the subject. I doubt not that my learned Göttingen friend had as high an opinion of the fact as I have.

\* Handbuch der Naturgeschichte, p. 242; Göttingen 1797.

Before I proceed any further in my reply to Mr. Blumenbach, I beg leave to trouble you with some facts which are a good deal similar to those related in my memoir. They certainly favour the system which I have advanced. Independently, however, of their connection with the subject of the memoir, they appear worthy of preservation; for they serve to illustrate, in some degree, the history of the manners and instincts of serpents; a subject which has been too much neglected by naturalists.

A species of coluber, which is commonly called the chicken snake in the southern parts of the United States, of which it is a native, frequently climbs up the loftiest trees in pursuit of young birds. One of my friends\*, when he was in Georgia, several years since, had an opportunity of seeing one of these snakes in a situation which furnishes a striking argument in favour of my opinion. The active reptile having seized upon a young martin† (which had left its nest for some days), upon a walnut-tree, at the height of about thirty feet, had not a little difficulty in swallowing the young bird. Having taken in the head first, as is commonly the case with our serpents, the bird made great resistance by the flapping of its wings, so that the serpent could only swallow the head and neck. Whilst the wings of the bird were in motion, numbers of the old martins collecting together flew about the snake and attacked him with their bills. Here, as in the instance related by Mr. Rittenhouse, the old birds were actuated by the instinct of saving their young.

The following facts were communicated to me by Mr. John Heckewelder:—"In the summer of 1770," says this gentleman, "while I was fishing under the bank of Lehigh‡, I heard, for the space of near an hour, the sound of a ground-squirrel, seemingly in distress, on the top of the bank. At length I went up to see what was the matter with the squirrel; when, to my utter astonishment, I discovered the animal about half way up a bush, but running sometimes higher up, sometimes lower down, and a very large rattlesnake at the root of the bush on which the squirrel was. Here I was immediately struck with the idea that the snake was in the act of enchanting, and I hoped now to become fully convinced that the rattlesnake obtained its prey altogether in this manner, as I had often heard reported. I therefore sat down quietly on a log about six yards distance, where I had

\* The ingenious Mr. William Bartram.

† *Hirundo purpurea*.

‡ The principal western branch of the Delaware, which runs by Bethlehem.

a full view of both the snake and the squirrel. Sometimes I thought the squirrel going down for the last time, and to enter the jaws of the snake; but it would again return up the bush with the same liveliness it had run down. Finding, finally, no material alteration in the squirrel or its motions, and my patience being exhausted, I determined on killing the snake, and examining into the case of the squirrel, viz. what strength, &c. it yet retained after being charmed for so long a time; for, by this time, the supposed charm had lasted near three hours. I struck at the snake with a long pole, but missed it; upon which it ran down the bank where I had been fishing. Remaining by the bush on which the squirrel was, I hailed a man on the opposite side of the river, desiring him to cross in a canoe and kill the snake under the bank. To which he immediately complied; but likewise missing his stroke, on account of the bushes, the snake took up the bank again, where I killed it. We now both joined to shake the squirrel down; but it had both sense and strength enough to climb to the very top, I suppose near twenty feet high. However, we brought it down to the ground; and though it had fallen about two yards from the bush, it well knew its hole in which it dwelt, and this was at the root of the bush, and exactly at the spot where the snake had lain. Here the mystery was cleared up to us at once. We conjectured that the snake was either watching for the squirrel to come down to enter its hole, or for its companion or young, which were probably in the hole, to come out; all of which were sufficient to cause anxiety in the squirrel on the bush. The dexterity, however, of the squirrel in making its way into the hole, and at the very place where we stood, showed plainly that it retained its full strength and sagacity, and had by no means suffered from the charm of the snake\*."

"A similar circumstance, to which I was also an eye-witness, happened," says Mr. Heckewelder, "in the year 1771, near Wyalusing, on Susquehanna, where the cries of the chewink† drew my attention to the spot. The rattlesnake was just entering a heap of brush, in which the old ones had their nest with young. I supposed that one or the other of the old ones, with the young, would have become its prey, had I not approached and relieved them by killing the snake‡."

\* Letter to me, dated Bethlehem, August 5th, 1796. Mr. Heckewelder has since informed me, that the snake, during the whole of the time he attended to it, never shook its rattle.

† The *Fringilla erythrophthalma* of Linnæus.

‡ Letter to me, dated August 5th, 1796.

The following fact is similar to one which I have related in my memoir\* :—“ In the year 1762, at Tuscarawas, on Muskingum, while going to fetch water out of the river, I observed,” says Mr. Heckewelder, “ a large black snake running out on a long limb of a large tree which stood on the water’s edge. This limb was nearly horizontal over, and about twelve feet above the level of, the water. I could not, at first, conceive what the snake aimed at, until near the end of this limb I saw the animal stretch downwards, where I observed a hanging† bird’s nest, pretty well concealed between some small boughs or leaves, into which the snake put its head, having strung its tail, with part of its body, round the limb above. Determined on killing the snake, if possible, I ran to the house for my gun, and shot the reptile, which fell into the river, with a young bird in its jaws.”

“ I and another person once observed a snake of this kind run up a tree pretty high, and put its head into a woodpecker’s nest, where, as we supposed, it sucked the bird’s eggs, it being too early for the young birds to be hatched ‡.”

On these facts I shall not trouble you with any comments, but shall proceed with my examination of Mr. Blumenbach’s Remarks.

[To be continued.]

XXXVI. *Observations on the different Theories of Philosophers to explain the Phænomena of Combustion.* By CHARLES PORTAL, Esq. §

THERE is no phænomenon in nature that has hitherto engaged the attention of philosophers with less success in the elucidation of its principles than that of combustion, and it at present affords one of the chief obstacles to the forming of a clear and satisfactory theory of chemistry. It is not singular, that a subject of so striking a nature, and abounding with a variety of such important phænomena, should have early attracted the attention of philosophers, and we accordingly find that it was made the subject of investigation by lord Bacon and Mr. Boyle.

These two philosophers were, however, more particularly confined in their inquiries to the nature of the unknown element called *fire*, nor did they reach sufficiently far in such

\* See pages 67 and 68,

† Letter already referred to.

‡ Oriolus Baltimore of Linnæus.

§ Communicated by the Author.

inquiries as to frame any particular theory on the subject of combustion.

In referring back to the earlier periods of the history of chemistry, we shall find that Becher was the first philosopher who withdrew that science from the contracted limits of pharmacy and alchemy, and laid the foundation of the doctrine of phlogiston.

G. Ernest Stahl, whose genius was formed for the highest improvement of science, succeeded him; and it is to this philosopher that we are indebted for the first positive attempt to explain the phænomena, and to exhibit a regular theory of combustion.

The doctrine of phlogiston, as expounded by this celebrated chemist, is too well known to require being described at any considerable length.

It proceeded on the assumption, that there was only one substance in nature capable of combustion, which he therefore called phlogiston; and he held, that all bodies that were inflammable owed their combustibility only to the presence of this principle. Combustion, therefore, he considered merely as its separation in the form of light and heat; and such bodies as were not inflammable were thought to be devoid of it: for during the combustion of substances he taught that their phlogiston flies off, and the incombustible parts of them alone remain behind. Thus, if iron be exposed to a sufficiently strong heat it will undergo combustion, a complete alteration will take place in its constituent parts, and a residuum will be found left of an incombustible nature.

Stahl explained these phænomena in the following way: Iron he considered as a peculiar earth united to a certain proportion of phlogiston; when it was made to undergo combustion, the phlogiston which formed a part of it, and to which it alone owed its combustibility, made its escape, and there was left behind only the base, which was found to be no longer inflammable. Now, as this separation was always attended with the emission of light and heat, phlogiston was considered only as heat and light combined with some other species of matter in a peculiar and unknown way.

This doctrine was considered as deriving much support from the fact, that a body, after having undergone combustion, (*i. e.* by the dissipation of its phlogiston in the form of light and heat,) was converted into a body that was no longer combustible, but which might again have its combustibility restored by the addition of any inflammable matter. Thus, in the example before adduced, if, after the iron has undergone the  
process

process of combustion, and formed a residuum that is incombustible, we heat this latter substance with charcoal (an inflammable body), the metal will be again revived—phlogiston is imparted to it from the charcoal, and we again procure a combustible substance. Thus, the light and heat which are evolved during combustion are supposed to proceed from the burning body, and to be occasioned by the separation of phlogiston from the base to which it is united.

Such is the outline of that theory of combustion the foundation of which was first laid by Becher, and afterwards reduced into a regular system by the immortal Stahl. The phlogistic doctrine of this chemist was universally adopted until the middle of the present century, when the discoveries of Mayow and Hooke, of Priestley and Lavoisier, led the way to a new theory of combustion, which soon displaced the former, and established itself on its ruins. Stahl, from being wholly occupied in demonstrating the existence of phlogiston, seems to have paid no attention to the influence of air on combustion. The experiments of Boyle and of Hales had already pointed out the influence of this element on many of the phenomena of combustion, and the increase of weight that bodies acquire during such a process, is a fact totally irreconcilable with the theory of phlogiston.

Many attempts, however, were made to overcome the weight of this objection by the disciples of Stahl; and they even had recourse to the supposition that phlogiston was the principle of levity, and that, when abstracted from any body, that body, by losing so much absolute levity, became heavier. So strongly were they fettered by the tenets of their master, that, without submitting to the labour of investigation, they implicitly adopted any illustration that was conformable to his ideas; affording a strong proof of the desire that pervades the human mind to reduce every thing to first principles, and to adopt hasty generalizations, without having recourse either to an extensive collection of facts or the more certain evidence of accurate experiment.

Whilst other chemists were intent on reconciling the various discoveries that had been recently made with the hypothesis of Stahl, Lavoisier (of a bold and creative genius) was led to call into question the very existence of phlogiston itself. This arose from his having discovered that during the process of combustion a portion of air constantly enters into union with the body which is made to undergo this operation, and that the weight of the air which disappears in the process is exactly equal to the increase of weight gained by the

the body that has undergone combustion. These important facts led to the adoption of a new theory of combustion well known at present by the name of its illustrious author.

The theory of Lavoisier is founded on the absorption of oxygen by a combustible body, and proceeds upon the following principles :

1st, That combustion never takes place without the presence of oxygen gas.

2dly, That in every combustion there is an absorption of oxygen gas.

3dly, That there is an augmentation of weight in the products of combustion equal to the gas absorbed ; that the oxygen likewise imbibed by the combustible body may be again recovered from the compound formed, and the weight regained will be equal to the weight which disappeared during combustion.

And, lastly, That in all cases of combustion there is a disengagement of light and heat.

These facts have been established by the most accurate experiments, and are too well known to require any further illustration ; they incontrovertibly prove the general principle, “that combustion is only a play of affinities between oxygen gas, caloric, and the base of the combustible body. It is a further part of this theory, that the light and caloric which are evolved during combustion proceed from the oxygen gas, and that they are not emitted, as should follow from the phlogistic doctrine, from the combustible body.

As this theory, however beautiful, is not capable of explaining with clearness and accuracy many of the phænomena of combustion, Dr. Thomson, of Edinburgh, has lately (see Nicholson’s Journal, New Series, for May and June 1802,) offered another which places this subject in a point of view somewhat different, and which certainly bids fair to enable us to estimate the phænomena of combustion with more success than has hitherto been done. Chemists have been lately accustomed to give to the term combustion, according to the foregoing theory, a new meaning, and to make it stand for the general combination of a body with oxygen. Nothing, however, can be more evident than the difference that in numberless instances prevails between the act of oxygenation of bodies and that of combustion, inasmuch as neither the phænomena attending them, nor the results arising therefrom, are the same.

It is probable that this error has arisen from the consideration that all bodies during their combustion combine with oxygen, without at the same time recollecting that this latter effect

effect may be produced without any of the phænomena usually attendant on combustion; and that, though certainly all combustion presupposes the combination of oxygen with a base, yet this combination may be, and repeatedly is, effected where no combustion can possibly take place.

It is the object, therefore, of Dr. Thomson's theory to point out the difference which in numberless instances prevails between the act of oxygenation of bodies and that of combustion, and particularly to account, in a more satisfactory manner than has hitherto been done, for the emission that takes place during combustion of light and caloric. The two following are the leading positions of the doctor's theory:

1st, That during combustion all combustibles emit light, which previously formed a necessary ingredient to their own composition: and,

2dly, That the heat which is evolved during the process of combustion proceeds from the decomposition of oxygen gas.

It has been before observed, that by the phlogistic theory the light and heat are supposed to proceed from the combustible body; but that by the theory of Lavoisier they are held to proceed from the decomposition of oxygen gas, of which body they are considered as forming constituent parts.

In the infancy of chemical knowledge, and before the discovery of vital air, that the extrication of heat proceeded from the combustible body, was the only natural conclusion that could present itself to the mind; and as light and heat were considered as only modifications of the same substance, the supposition of course prevailed that both were evolved from the inflammable body.

Since, however, the later experiments of philosophers, and particularly those of Drs. Herschel and Woolaston, have disproved this supposed identity, it evidently becomes no longer necessary to trace these two substances to the same source; and this, as is before stated, is the case with the theory we are now reviewing.

That the caloric which is evolved during combustion should proceed from the decomposition of the oxygen gas, is, amongst a variety of other reasons, rendered the more probable from the consideration that bodies possess a greater share of caloric in the gaseous state than in any other; and consequently the probability is greatly strengthened, that the heat which is evolved during the process of combustion proceeds rather from the oxygen gas than from the inflammable matter, and that it is from the condensation of oxygen gas that caloric is chiefly evolved; or, according to the explanation of Lavoisier, that the oxygen of the gas, possessing a stronger affinity  
for

for the base of the combustible body than for caloric, is thereby attracted, and that the heat combined with it is consequently set at liberty, and diffuses itself among the adjacent bodies.

To the second position of Dr. Thomson, that the light invariably proceeds from the combustible body, and not from the oxygen gas, there are much stronger objections, and such as, with our present collection of chemical facts, are, I apprehend, not readily to be explained.

It is a part of the theory of the doctor, that oxygen gas possesses no light: indeed this is so material a part, that, unless it can be substantiated, the theory is inadequate to elucidate the phænomena intended by it.

How shall we, however, reconcile the following facts on the above supposition? If nitric acid be exposed to the light, it changes colour; it first grows yellow, afterwards green, and lastly red, and oxygen gas is disengaged; and on examining the acid we find that it is converted from nitric into nitrous acid.

It is evident that, as this decomposition is of a chemical nature, the light that occasions it either combines with the oxygen to form oxygen gas, or with the acid to form nitrous acid: that the latter is not the case we are justified in supposing, because we find no difference between acid so procured and that gained by any other means, and we are necessitated to conclude that the light has combined with the oxygen, and that the latter is by this mean converted into oxygen gas. The same reasoning may be applied to the conversion of oxygenated muriatic acid by light into simple muriatic acid. Unless these facts can be otherwise satisfactorily accounted for, the position of Lavoisier still remains with all its force, that light is a constituent part of oxygen gas. That light forms a necessary part in the constitution of many bodies, is a fact too well authenticated to be refuted. The experiments of the Dutch chemists, who heated together the filings of different metals with sulphur under mercury; the phænomena of the pyrophori; the inflammation that takes place from the action of many of the acids on the oils; and the recent experiments of Dr. Hulme, illustrate this fact beyond the possibility of contradiction.

After a body has undergone combustion, we learn from this theory that it is deprived of light, and that it is only by means of a combustible body that light can be again transferred to the product of combustion. Thus water is considered by Dr. Thomson as a product of combustion, and consequently deprived of light. If, says he, we decompose  
this

this water by means of iron or zinc, we restore it to its former combustibility by occasioning it to imbibe light, which is afforded it by the metal. If such decomposition is effected by means of iron, 73 parts of this latter unite with 27 of oxygen. Now, as every 27 parts of oxygen require about four of hydrogen to form water, of course these four parts of hydrogen are liberated during such decomposition: but, as it is not probable that combustibles are capable of combining with light in all proportions, it may be asked, Whether the 73 parts of iron which are oxidated contain just light enough, and no more, to restore the combustibility of the four parts of hydrogen? for, if there be too much for that purpose, the superabundant quantity ought to become visible; and if too little, a part only of the hydrogen should recover its combustibility, and be converted into gas. The same reasoning may of course be urged with regard to the decomposition of water by any other metal; for it is somewhat singular, that the combustible should always contain and give out the precise quantity of light that is sufficient to restore combustibility to the base of the product of combustion, and in no case either more or less. These, as well as some other objections to the above theory, were pointed out by me in a paper entitled *Remarks on Dr. Thomson's Theory of Combustion*, and inserted in *Nicholson's Journal* for the month of July 1802.

It should appear, then, that we are not in possession of a sufficient collection of facts to enable us to form any theory of combustion that is perfectly free from objection, and calculated to explain all the phænomena attendant on it. Whether the light evolved during combustion is emitted from the combustible body or from the oxygen gas, must still be considered as a matter of doubt; for the supposition seems perhaps the more probable, that it is partly derived from both. The subtle nature of light itself is highly adverse to any inquiry into its real essence. The same observation will likewise apply to caloric. The theory last noticed is, however, possessed of a high degree of merit; it satisfactorily explains many phænomena that were before but little understood; it points out, with sufficient perspicuity, the difference that prevails between the act of oxygenation in bodies and that of combustion; and though certainly liable to some objections, it approximates the nearest to a satisfactory explanation of the phænomena of combustion of any that have hitherto been offered.

CHARLES PORTAL.

\* \* \* Mr.

\* \* Mr. Portal is probably not aware that the theory which he attributes to Dr. Thomson as its author was first proposed by Dr. Gren, whose doctrine is briefly as follows:

*Oxygen gas is composed of oxygen and caloric—Combustible bodies consist of their respective bases in chemical combination with the matter of light—Free light consists of the matter of light united to caloric.*

Combustion, then, is a mere play of chemical affinities; the oxygen of the oxygen gas uniting with the base of the inflammable body to form an oxide or an acid, while the caloric of the gas unites with the matter of light of the combustible body, and forms free or visible light. By this theory, no combustible substance can be a simple body—by Lavoisier's, they may.—See Gren's *Principles of Modern Chemistry*, English edition, chap. ii. articles *Caloric, Light, Phlogiston, Combustion*.—A. T.

XXXVII. *History of Astronomy for the Year 1802. Read in the Athenæum of Paris December 30, by JEROME LANDE.*

[Concluded from p. 179.]

ON the 7th of June I published the two last volumes of the large *History of the Mathematics* by Montucla. The fourth is almost entirely devoted to the history of astronomy and navigation, which I communicated to the present period.

There has appeared also, in two volumes quarto, with 23 plates, *Histoire de la Mesure du Temps par les Horloges*, by Ferdinand Berthoud, mechanist of the marine, member of the National Institute, and of the Royal Society of London. It contains a description of escapements, compensation balances, moving spheres, and of the principal inventions of clock-work. The author speaks of all the celebrated clock-makers, except Louis Berthoud, his nephew, to whom we are indebted for all the time-keepers made since 1784. I shall here take occasion to observe, that Mr. Emery died at London, and that his widow has four time-keepers which she wishes to dispose of: they would be a valuable acquisition.

We have received the 7th volume of the *Transactions of the Royal Irish Academy*, published in 1800, and in which Mr. Young examines the solution given by Newton of the problem respecting precession of the equinoxes; also the 8th volume of the *Irish Transactions*, Dublin 1802.

On the 18th of July my small tables of logarithms appeared: they are the most convenient and the most correct yet given.

The third volume of the *Mécanique Céleste* of C. Dela-place has been printed, and was published on the 29th of December. This work will form an epoch in physical astronomy. It exhibits a series of important researches by that great geometrician, and the perturbations of each of the planets by the action of all the rest; with a new theory of the moon, containing, besides other things, the discovery of a new inequality the period of which is 180 years, and of which I have already spoken.

M. Hassenfratz has published his *Cours de Physique Céleste*, or *Leçons sur l'Exposition du Système du Monde*. He makes no mention in it of my Astronomy, but he nevertheless has borrowed from it things which he could not find any where else. I have corrected some faults in it. (*Bibliothèque Française de Pougens.*)

The Board of Longitude has sent to the printing-office observations made for two years with the new instruments of the observatory by Mechain and Bouvard, to be printed in folio like those which Monnier published between 1751 and 1773, and those of Greenwich in England, which were worthy of serving as a model.

On the 3d of July M. de Rosel, a naval officer, arrived at Paris with the journals of the voyage undertaken in search of La Perouse, under the command of captain d'Entrecasteaux.

La Grandiere also has brought a journal of that voyage, which the English government had a year in their hands, but which they restored to him. At the dépôt of the marine there are 58 charts constructed during that voyage by Beautemps-Beaupré, hydrographer of the expedition.

In the month of June I received intelligence from C. Bernier, who was at New Holland. Captain Baudin touched only in two places in an extent of 400 leagues which he traversed on the western coast. He was preparing to accomplish the remaining part of the expedition to the north and south: but it appears to me that the zeal of this astronomer has been thwarted by the indifference of the captain; and this voyage, on which we founded great hopes, will not be so productive as it ought to be.

The ship Lady Nelson, sent out with the Investigator, captain Flinders, which arrived at New Holland about the middle of December 1801, fell in with the Geographe, commanded by Baudin. She then found at Port Jackson the Naturaliste, commanded by Hamelin, and which sailed about the middle of May to go in search of the Geographe, from which she had been separated by a gale of wind in the Straits

Straits of Basse. Captain Flinders has sent home his observations, and a chart of those parts of New Holland which he visited.

Captain Flinders made a discovery between Van Diemen's Land and New South Wales, of which information was received in March 1802: Governor King's Island, Cape Alban Otway, Portland Bay, Cape Northumberland.

Joseph Joachim de Ferrer has sent me several positions observed on the Mississippi and on the Ohio, which will be of the more importance as the French government is employed with the means of deriving benefit from that new colony; which may be attended with immense advantages, as Raynal has shown in his *Histoire Philosophique*.

The Voyage to the Northern Part of Asiatic Russia, the Frozen Ocean, the Sea of Anadir, and the Coasts of America, from 1785 to 1794, by commodore Billings, translated by Caſtera, in two volumes quarto, has made us acquainted with countries before undeſcribed or defined. The ſhores of the Kouſina have been traversed: a chart by M. Bauer, and Arrowſmith, the Engliſh geographer, is added. The officers complain of the commander, who would not allow them to penetrate further north, notwithſtanding the inſtructions he had received to ſearch for a paſſage through the Frozen Ocean.

Travels into the interior Parts of Africa in 1797 and 1798, by Frederic Horneman, has been published. Horneman proceeded from Cairo to Mazoul. Another French edition, with additions by C. Langles, is in forwardness.

Geography has been enriched also by *A Voyage to Senegal* by C. Durand, one volume in quarto, with many plates. The author was a long time at Senegal; and I have mentioned his researches in a memoir on Africa.

M. Sutzen, a physician, accompanied by M. Jacobzen, a surgeon, is also preparing to penetrate into the interior parts of Africa. They have been furnished with observations by baron Von Zach, of Gotha. The duke of Gotha has given them a sextant of seven inches radius, and a time-keeper by Emery; three artificial horizons, with their levels; an achromatic telescope of twenty inches, and two and a quarter inches aperture; a declination compass, a mariner's compass, and a camera obscura.

M. Sutzen will proceed from Constantinople to the eastern coast of Africa, where he intends to join some of the caravans of Zinguebar or Monœmugi. He hopes to be four or five years absent. (Von Zach's Journal for August 1802.)

On the 17th of July we saw don Domingo Badia, com-  
P 2 military

military of war in Spain, who travels by authority of the Prince of Peace, accompanied by Roixas, a naturalist. He will embark in England for Morocco, and will follow the route of the caravans of the interior.

*Voyages à Madagascar, aux Indes Orientales, et à Maroc*, by Alexis Rochon, a celebrated astronomer and geographer, have appeared in three volumes.

The general depôt of war at Paris continues its labours with surprising activity, as may be seen in the *Moniteur* of June 25 and October 16. Generals Calon, Clark, and Andreoffy, who had the direction of it in succession, have been succeeded by general Samson.

The map of the country between the Adige and the Adda will soon be followed by maps of all Piedmont, of the Italian republic, and of the island of Elba: those of Bavaria, Helvetia, of the Valais, and of the four departments of the Rhine, are continued. More than a hundred geographical engineers are employed, among whom are the celebrated Nouet, Henry, and Tranchot.

On the 1st of July M. Henry quitted Bavaria, disgusted by the opposition of M. Bonne. The latter is still occupied with the map of Bavaria; but we lose the measurement of a degree, which M. Henry proposed to make. However, we shall have from the Scheldt to the Adige, and from Brest to Munich, an uninterrupted trigonometrical outline, which will soon be filled up with a complete topography. A map of Egypt in fifty sheets, and those of the Morea and neighbouring isles, are now engraving. Materials are collecting for maps of Saint Domingo and of Louisiana.

The beautiful map *des chasses*, a master piece of engraving, of which only five sheets were terminated, will be carried to thirteen.

A plan is even in agitation for making a new and correct edition of the large map of France in 183 sheets, which had been too much neglected. In the mean time, thirty are retouching.

The collection of manuscript charts at the depôt, which already amounts to 7400, daily increases. C. Barbier-Dubocage, already known by his important labours, turns them to the best advantage possible; and the depôt of war, encouraged by the government, is about to produce to geography immense riches.

The first number of a topographical and military memorial, edited at the general depôt of war, has been published for November 1802. It contains a notice respecting the geographical charts; a treatise on geodesic operations; tables for reducing

reducing the angles of one plane to another; the determination of heights by the barometer.

C. Nouet, who arrived from Egypt on the 5th of January, has already set out for Mount Blanc, where he will continue the laborious operations which he began in 1796.

The Russians are going to construct a map of Esthonia and Volhynia.

M. Weifs has terminated his map of Swisserland in sixteen sheets.

A beautiful chart of the southern part of the geometrical survey, by captain Mudge and Mr. Dalby, has been engraved in England in four sheets.

M. Ciccolini has made a tour to the coasts of the Adriatic, where he has verified the positions of a great number of points. In regard to Pezaro there was an error of 30'' in time.

The travels of baron Von Zach, so useful to the geography of Germany, are detailed in the excellent journal which he publishes every month.

M. Mentelle has undertaken for the First Consul a terrestrial globe a metre in diameter, which will contain all the new discoveries collected with great care and erudition.

On the 26th of March I obtained permission to found at the Institute a prize, to be assigned to the person who, in the course of the year, shall make the most curious observation, or publish the most useful memoir, in regard to astronomy.

C. Chaptal, whom we have the good fortune to have for minister of the interior, and whose labours as a philosopher announce his zeal as a minister, has attached calculators to the Board of Longitude; he has granted gratifications to C. Flaugergues, an assiduous observer, and to C. Pons, keeper of the observatory of Marseilles, who discovered a second comet.

When the consultation established for distributing rewards, and which was very useful in 1793 and 1794, was suppressed, the Institute was charged to take up the same business, and to propose to government the gratifications it would be necessary to give. The Institution has not yet exercised this right, but I have proposed to it to fulfil its obligation.

M. Jacotot, professor of astronomy at Dijon, has obtained from C. Giraudet, prefect of La Côte-d'or, the repairs necessary for the observatory damaged by a fire. I hoped I should have been able to observe there the eclipse of the sun on the 28th of August: but the sky was absolutely cloudy, as it was at Paris. We have received observations of that

eclipse from C. Thulis at Marseilles, and from C. Flaugergues at Viviers.

Prince Henry of Wirtemberg, brother of the empress dowager of Russia, who resides at Hamburgh, has purchased the beautiful instruments made by Megnié, of Paris.

The speculum for a telescope, which Dr. Herschel sent to Petersburg, has been sent back to him for the purpose of being mounted. Kramp solicits for the establishment of an observatory at Cologne.

The academy of Turin requests the re-establishment of its observatory; and M. Henry has offered to take the direction of it.

Fathers Canovai and Delricco, who have at Florence the observatory of father Ximenes, have sent us a list of their instruments, to prove that they cannot exert themselves with any advantage for astronomy; but they have published tables of logarithms.

The king of Etruria has announced that he still entertains a design of placing an astronomer in his *Cabinet de Physique*, where he has already some excellent instruments.

General Chabert, who has returned from England after an absence of ten years, has been associated with the Board of Longitude, to which office he was justly entitled by his geographical labours; and his age of sixty-eight years does not prevent him from being present at the sittings, and from being useful by his advice and experience.

The youngest son of our celebrated astronomer C. Mechain has been appointed by the Board of Longitude secretary of the observatory, which affords him an opportunity of devoting himself to astronomy. Augustine Francis Mechain was born at Paris, March 5, 1786.

He has succeeded his brother Isaac, who having returned from Egypt has been appointed commissioner of foreign relations at Galassia, in Moldavia.

C. Lechevalier has constructed in the Hotel of Foreign Relations a small observatory, where he can gratify the taste and curiosity of which he gave proofs during his stay at Constantinople.

C. Louis Berthoud has constructed for the observatory a clock worth a thousand crowns. The pivots turn in rubies, and we hope it will equal that given by the First Consul to the observatory of Milan.

C. Pons, a watchmaker of great skill and ingenuity, made an experiment at my observatory of a clock which beats half seconds with a free scapement, and which goes with wonderful regularity.

C. Leveque

C. Levêque has published, in the fourth volume of the *Memoirs of the Institute*, a learned paper on the longitude, and particularly on Maignon's charts for reducing the moon's distances observed at sea.

C. Richer has made a new trigonometric compass, or compass for reducing the moon's distances from the stars. It has ingenious inventions for dividing into unequal parts the rules containing the distance, the sum, and the difference of the heights. In my short *Treatise of Navigation* I have given a description of the instrument which gained the prize in 1791; and in the *Connoissance des Temps* for the year 4, the demonstration of the formula of M. de la Grange, which serves as a foundation for this instrument; which, however, has been much improved: the only inconvenience is, that it will cost 600 francs.

Mr. Troughton has finished the model of Menozza's circle, which gives the double of the multiple of Borda's circle, even retaining the smallest fixed mirror.

A curious detail respecting the calendar of the Indians has been published in the letters of the abbé Sevin.

C. Girard read in the Institute a long memoir on the Egyptian milometer, the value of which he determined in the island Elephantine, on the very spot, 19 feet 5.6 lines; which shows that the antient measurement of the earth by Eratosthenes was very correct.

The abbé Testa has published at Rome a dissertation on the zodiac found at Dendara (in Greek, Tentyris) in Egypt. He undertakes to prove that it is not older than 300 years before the vulgar æra. The History of Herodotus, translated by Larcher, edition of 1802, contains a fally against the unbelievers, who carry back the period of one of the zodiacs of Tentyra, at present Dendera, and of Esné, or Henne, to 6000 years; and the author's only reason is, that it would be 217 years before the creation.

He adds a notice by Visconti, who says that the first sign of the great zodiac is Leo; that Libra, the symbol of the equinox, is in its place; and that the resemblance of the greater parts of the signs to the Grecian proves that this zodiac was constructed at a time not so remote as the earliest periods of the Greek astronomy: he is almost convinced that this work belongs to the first century of the vulgar æra.

The exterior cornice exhibits a large Greek inscription, which may decide the question; but another Greek inscription contains Roman names, and announces a Cæsar, who could be only Augustus or Tiberius. In a word, M. Visconti says that the architecture of the temple of Tentyra, though in

the Egyptian taste, and though some hieroglyphics are carved out on the walls, has an evident relation to the arts of Greece. For my part, I have remarked by the engraving of M. Denon that Cancer is inscribed in two lines, at the beginning of the descending signs and at the end of the ascending; which proves that the solstice was towards the middle of Cancer; and this carries it back 3000 years. But I have shown in my *Astronomy* that Eudoxus, who wrote 370 years before our æra; and Aratus, who followed Eudoxus, described the sphere according to an older tradition, which goes back twelve or thirteen hundred years before the vulgar æra, and which came from Egypt or India. Petau, Whiston, Frerët, and Legentil, found nearly the same date: it is therefore natural to believe that it should be found in the zodiac of Tentyris, which consequently, in this respect, may be considered as a work of the Greeks.

C. Villoison, well known by his Greek erudition, has celebrated astronomy in Latin verses, which announce both his talent for Latin poetry and his taste for astronomy. He addressed them to me on my birth-day, and published them in the *Magazin Encyclopédique*. In the notes he speaks of the labours of the uncle, the nephew, and niece; and of citizen Burckhardt, their most learned co-operator and their most intimate friend.

C. Boulage, of Troye, has written a beautiful epistle to Piazzzi on the discovery of his planet: it has been inserted in the fourth number of the *Memoirs of the Academical Society of the department of Aube*.

Astronomy this year has lost the respectable Augustin Darquier, member of the National Institute. He was born at Toulouse, November 23, 1718, and died on the 10th of January 1802. He devoted himself early to astronomy through natural inclination. Though in a town distant from the capital, he applied to it with a zeal and activity which were not lessened by age. He purchased instruments, established an observatory in his own house, and caused two volumes of observations to be printed at his own expense. His translation of Lambert's *Cosmologic Letters* has been printed at Amsterdam. He instructed pupils, paid calculators, and being in no need of the assistance of government he was indebted for every thing to himself. I have printed his last observations in my *Histoire Céleste*; they go as far as the 19th of March 1798; and he had some more to send me, though 80 years of age. The Lyceum of Toulouse will give us some further particulars respecting his long and interesting career.

Astronomy lost also in France, on the 1st of March, M. Lemery, whose singular turn for calculation I discovered thirty years ago. Being then attached to the marquis De Pufieux, he employed all his leisure time in calculation. I made him calculate a great many places of the moon, which were published in 1777 in the *Connoissance des Temps* for 1779: and for fifteen years he made all the calculations of that work with equal care and assiduity.

Daniel Bogdanich died at Pest, in Hungary, on the 31st of January, at the age of 37. He was occupied with the geography of Hungary. (See Von Zach's Journal for April 1802.)

M. George Frederic Kordenbusch, astronomer of Nuremberg, died on the 3d of April, at the age of 71.

In the month of September baron De Vega was drowned in the Danube, and it is supposed threw himself into that river. We are indebted to him for the large edition of Vlacq's tables of logarithms in folio, containing the numbers to 100,000; and the fines from ten to ten seconds, which had become very rare, and in which he corrected many faults.

Meteorology has this year exhibited very extraordinary phænomena. In the month of January an extraordinary inundation: the Seine rose 22 feet and a half above the height of 1719.

On the 25th of May a frost, which did immense hurt to the productions of the earth.

On the 14th of May snow fell at Munich, and continued for forty-five hours: the trees were stripped of their leaves, and overturned.

In the months of July and August an excessive heat took place, and continued forty days. The thermometer at the observatory stood at 97° F., which is very uncommon at Paris.

The need of having meteorological rules founded on observation, induced our learned minister to establish a correspondence on that subject, and an office to conduct it.

Meteorological observations have been made in thirty places, from the convent of Mount Cenis as far as the borders of the sea.

John Dominic Beraud, born in 1741, formerly a draftsman at Coni, and who for twenty years has resided at Turin, continues to employ himself in making meteorological observations, and sends us regularly his results.

Lafon, geographical engineer in Louisiana, has sent us meteorological observations made at New Orleans; and we have received some made at Guadaloupe by C. Hapel Lachenaye

chenaye between the years 5 and 9. The fourth volume of the Memoirs of the Institute contains some also.

C. Coulomb read in the Institute experiments on the method of magnetizing needles to saturation; with a comparative view of the methods of Knight, Duhamel, and Æpinus. He has pointed out to navigators the surest means of obtaining the best needles. The broadest and largest are preferable, but they must not be thick. This philosopher has published in the fourth volume of the Institute a curious memoir on the dipping needle.

The declination of the magnetic needle at Paris was observed by C. Bouvard at the observatory, on the 2d of May, to be  $22^{\circ} 3'$ , and on the 22d of July  $21^{\circ} 45'$ .

C. Lenoir, at the Garden of the *Dépôt de la Marine*, found it on the 20th of June to be  $22^{\circ} 6'$  with a compass, on which he employed every care possible. But the changes which take place at the different hours of the day, and in different months of the year, amount to more than  $10'$ . We can therefore only say that the declination is  $22^{\circ}$ , and it was found to be the same in 1792 and 1800; so that at present it appears to be stationary, whereas ten years ago it increased from  $6'$  to  $8'$  annually.

Forty years ago I observed it to be  $18\frac{1}{4}^{\circ}$ . (See *Connoissance des Temps* for 1762.)

M. Simonin, professor of Croisic, has sent us the result of a thousand observations on the tides, with the tables necessary for keeping an account of the variations produced by the sun and moon according to their altitude and distance.

M. Romme, professor of Rochfort, has sent to the Institute observations made every three minutes from low water to full moon to make known the irregularities; which are very singular; and a new table of the retardation of the tides, deduced from an immense number of observations.

He has presented also a general table of the tides, the currents, and winds, observed in all the seas of the earth; the publication of which will form an important supplement to the large Treatise on the Flux and Reflux of the Sea, which I published in 1781.

Mr. Grenier, an officer known by his discoveries in the Indian seas, has written a considerable work on the winds and currents in all the seas; with a theory which renders the explanation of them easier. He proposes to lay it soon before the public.

XXXVIII. *On the Disappearance of Saturn's Ring in the Year 1803. By Professor J. E. BODE\*.*

THE plane of Saturn's ring, as is well known, during the whole of his revolution of thirty years round the sun, retains its parallelism, and intersects the plane of the ecliptic in 17 degrees of Pisces and of Virgo at an angle of  $31\frac{1}{2}^{\circ}$ . The necessary consequence, therefore, is, that the northern side of the ring is illuminated for fifteen years by the oblique rays of the sun, and then the southern for the same period. It must also happen, that in the above places the plane of the ring passes twice through the sun in each revolution, at which time the edge of the ring only is illuminated. But the edge or thickness of the ring being too small to admit of its being seen from the earth on account of the great distance of Saturn, the ring becomes invisible. For fifteen years the earth and the sun are on the same side of the ring, which is the illuminated side, and therefore the ring during that period is always visible; but a little before, and at that period when the plane of the ring passes through the sun, or when Saturn's heliocentric place is in  $17^{\circ}$  of  $\text{æ}$  or of  $\text{m}$  in the ascending or descending nodes of the ring, the earth and the sun may first be on one side and then on the other side of the ring. In the first case, as long as its plane does not pass through the sun it will be visible from the earth; but in the other case it will be invisible. The earth, therefore, can then pass several times through the plane of the ring, at which time the ring will appear like a right line, or be entirely invisible; the consequence of which is, that the ring will be alternately visible and invisible.

This alternation of disappearing and reappearing will be exhibited by the ring in the year 1803. The sun, having illuminated the south side of the ring since October 1789, will pass through the plane of it in the month of June 1803, and will illuminate the northern side, when Saturn passes heliocentrically the ascending node of his ring; and in the mean time the plane of the ring will pass three times through the earth, which will be first on the side illuminated by the sun, and then on the opposite or darkened side; in consequence of which the ring seen from the earth will be twice visible and invisible in the course of a few months.

These phænomena will be better comprehended by inspecting the annexed figure, which supposes the observer to be

\* From his *Astronomisch Jahr Buch* for 1803.

placed in Saturn. This figure, then, exhibits the exact inclination of the plane of the ring to the ecliptic; the apparent motion of the earth and the sun, as seen from Saturn, together with the time of the passage of these planets through the ascending node of his ring. (See Plate V. fig. 2.)

AB is the ecliptic seen from Saturn in the sign of Pisces, and divided into degrees. The ascending node of Saturn's ring seen from thence is placed in  $17^{\circ} 6'$ , in which point the plane of the ring intersects the ecliptic at an angle of  $31^{\circ} 20'$ . The eye is then in that plane. Thus the ring appears as a straight line; *nn* is the north side, and *zz* the south side of it. The dotted line CD is the solar orbit seen from Saturn; and the sun's place is marked in it on the first day of each month, from November 1802 to October 1803. These places of the sun are directly opposite to the heliocentric places of Saturn: the longitude thus differs six signs, and the northern latitude of Saturn seen from thence changes into the southern of the sun. In this manner, the sun in November, December, January, and to the month of June, illuminates the south side of the ring; but always in a fainter manner, the nearer he comes to the plane of it. On the 15th of June he passes through the plane in *e*. The ring at that time is illuminated on the edge, and can be seen only by powerful telescopes as a fine luminous line.

The longitude of the point *e*, reduced to the ecliptic, falls in *f*, or  $20^{\circ} 42'$  of Pisces. The sun, then, about the middle of June begins to illuminate the northern side of the ring; in July, August, September, October, &c. he recedes more and more from the plane of the ring, and the illumination of it then becomes stronger.

The elliptical line is the earth's orbit as seen from Saturn, according to its direct and retrograde motion from the 1st of November 1802 to the 1st of October 1803; and its place is marked on the first of each month. Its longitude is six signs less or more than the geocentric longitude of Saturn; and the northern latitude of Saturn is changed into the southern of the earth.

Now, as the figure clearly shows, the earth in November and December 1802 is represented on the south side, or turned towards the sun, and therefore on the illuminated side of the ring. But afterwards the earth approaches more and more to the plane of the ring; and, as the ring receives the solar rays in a more and more oblique manner, the ring not only becomes narrower but even decreases in light. On the 13th of December, as may be seen by this small figure, the earth enters the plane of the ring, and passes to the  
northern

northern side of it, which is turned from the sun. The ring then, for the first time, becomes to us invisible: but this invisibility is not of long continuance; for, as may be seen by the figure, the earth on the 1st of January, by its Saturniocentric apparent retrograde motion, passes a second time through the plane of the ring, and goes to the southern or illuminated side, and the ring then begins to appear again as a straight line. In February, March, and April, the earth recedes more and more from the plane, and the ring becomes broader and more perceptible. But the sun during these months approaches nearer to the plane of the ring, and by these means it is illuminated in a more oblique direction. In the month of May the earth is at its greatest distance from the plane of the ring, while the sun approaches it more and more till the middle of June, when he passes through the plane in  $e$ , and the ring for the second time becomes totally invisible to the earth, which is then on its dark side. From this period, during the month of July and till August, the ring will remain in this state. About the 18th of August the earth will pass, for the third time, through the plane of the ring, as it will then be on the northern, which is now the illuminated side of the ring: it will again appear as a straight line, and be visible when viewed through good telescopes: it will increase in light and breadth in September and October in proportion as the earth and the sun recede from the plane.

Between the 1st of November 1802 and the 1st of October 1803 the ring of Saturn twice disappears and twice reappears. At the first disappearance on the 13th of December 1802, Saturn in the morning will be in the eastern part of the heavens; and the first reappearance will be about the 7th of January 1803; Saturn in the night-time being on the same side of the heavens.

At the second disappearance in the middle of June, Saturn in the night will be in the west, and, as he sets half an hour after midnight, the phenomenon may be observed. But at the second and last disappearance in August, Saturn will be after the sun, and therefore the phenomenon cannot be seen. In the month of October, when Saturn emerges from the sun's rays, he may be seen before sun-rise in the east, and the ring then will be very visible.

In regard to the above calculation of the times of the disappearance and reappearance of Saturn's ring, the accuracy of them, and the correspondence of them with the phenomena, must depend on the accuracy of the tables and the theory hitherto adopted in regard to the position of the ring.

The state of the atmosphere also, and the different power of the telescopes and of the eyes of the observers, may occasion some variation. The disappearance and reappearance of the ring, which are occasioned by the earth passing through its plane, can be better and more conveniently observed than those which take place in consequence of the passage of the sun through that plane, because the earth, on account of its quicker motion, passes sooner through the plane of the ring than the sun.

As the six first satellites of Jupiter move in the same plane with the ring, when the ring disappears, or becomes like a luminous line, these satellites must appear on both sides of Saturn in a right line, and can then be more readily discovered and distinguished from fixed stars.

XXXIX. *On the Disappearance of the Ring of Saturn.* By VAN BEECK CALKOEN, *Professor of Astronomy at Leyden* \*.

THE plane of Saturn's ring, according to astronomical observations, has always a direction parallel to itself, so that it intersects the ecliptic in  $17^{\circ} 18'$  of  $\text{♊}$  and  $\text{♋}$ , at an angle of  $31\frac{1}{2}^{\circ}$ , and the orbit of Saturn at  $20^{\circ} 42'$  of  $\text{♊}$  and  $\text{♋}$ . Now, if Saturn as seen from the sun be in longitude  $20^{\circ} 42'$  of  $\text{♋}$ , the plane of the ring will pass through the centre of the sun; by which the edge of the ring being illuminated, while the northern and southern surface receive no light, it must become invisible, and can be seen only by the most powerful telescopes as a fine luminous line. On the 15th of June next Saturn will be in this situation; and as this planet employs about thirty years in its revolution round the sun, this phenomenon takes place only once in 15 years. Other phenomena, however, in consequence of which the ring before the 15th of June will alternately disappear and reappear, are connected with this state of the planet. These phenomena arise from the different positions of the earth, which in the course of its revolution is at one time above and at another below the plane of the ring; so that from the 1st of November 1802 to October 1803 the ring will be twice invisible and twice visible; first the southern and then the northern, then the southern and afterwards the northern surface will be alternately seen from the earth within the course of ten months. For determining and calculating these phenomena with more accuracy, Lambert in-

\* From *Algemeene Konst en Letter-Bode*, No. 13, for 1803.

vented a very ingenious kind of projection, a representation of which was given by professor Bode, of Berlin, in his *Astronomical Almanac* for 1789, in which year these phænomena took place in the same manner as they will do in 1803; and this representation has been repeated in the same work for the present year: but as this diagram, in my opinion, is too difficult for those not well versed in astronomy, I flatter myself that the following figure will render the phænomenon easier to be understood by amateurs; though Lambert's method is more exact, and better suited to astronomers.

The circle (Plate IV. fig. 1.) represents the earth's orbit, and exhibits the points where the earth is placed at the commencement of each month. The vernal equinox is marked  $\odot \Upsilon$ , the line SZ drawn through the sun; and the place of Saturn on the 15th of June is the direction of the plane of the ring. Saturn, at the four periods announced, is in the lines drawn parallel to SZ, though at a distance from the sun five times greater than is here represented for want of room. The plane of the paper is that of the ecliptic, with which the one-half of Saturn's ring makes an angle of  $31^{\circ}$ .

I have already observed that the ring twice disappears, and then twice becomes again visible. This phænomenon arises partly from the situation of Saturn in regard to the sun, and partly from that of the earth; and each of these causes must be considered separately to have a clear idea of these phænomena. In regard to the first, it is evident that that side only of Saturn's ring which is turned towards the sun can be illuminated; and this illumination is strongest when the planet is in  $20^{\circ}$  of  $\pi$  and  $20^{\circ}$  of  $\uparrow$ , the whole surface of the ring being then opposite to the sun. Now as the direction of the ring is always the same, that is to say, parallel to itself, the rays of the sun, as the planet approaches to  $20^{\circ}$  of  $\pi$  and  $\times$ , must fall more obliquely on the ring, by which means it will be more faintly lighted, and at the same time appear smaller; and when Saturn has attained to this longitude, no part of the upper surface, but merely the edge of the ring, the plane of which passes through the sun, will be illuminated; so that the ring will then have the appearance of a thin luminous line, but will not be visible without the help of good glasses. For the last seven years the ring has been observed to decrease in light and in breadth; and this decrease will continue till the 15th of June, after which both will again increase. During the last fifteen years the southern surface of the ring has been illuminated, but in the next fifteen the northern surface alone will receive the rays of the sun. In whatever part of our solar system this phænomenon

non be viewed, the appearance will be the same. The ring till the 15th of June will continually become smaller, and on that day will be converted into an almost imperceptible line of light. The disappearance then of Saturn's ring on the 15th of June, or rather its decrease till that period and its subsequent increase, arises alone from the relative position of Saturn and the sun. As to the other causes of disappearance, which are the consequence of Saturn's position in regard to the earth, we must first keep in view the slow motion of Saturn compared to that of the earth, as this planet advances scarcely  $12^\circ$  in the time that the earth performs its revolution around the sun. On the 15th of June, at which time Saturn will be in  $20^\circ$  of  $\kappa$ , and the plane of the ring will pass through the sun, the intersection of that plane with the ecliptic, or the line ZS in the figure, passes through the middle of the earth's orbit. During the last fifteen years, while Saturn advanced from  $\gamma$  to  $\mu$ , this line of the plane of the ring was without, and above the earth's orbit, and during that period the southern and luminous part of the ring was always visible; but about the middle of December 1802 this line approached the earth's orbit, and on the 13th of December passed through it exactly in the point where the earth then was. The earth being then in the plane of the ring, the thickness of the ring only could be visible to us; and thus, for the first time in fifteen years, disappeared on the 13th of December 1802. On the other hand, as the earth moved straight towards Saturn, or parallel with that line, while Saturn advanced, it was necessary that the southern surface of the ring hitherto visible should become invisible, and the northern, which was not illuminated, being turned towards the earth, the whole ring would become invisible. But as the earth proceeded in its orbit it would soon reach the line of the plane, and on the 7th of January 1803 would again pass through it. At that period, the southern surface of the ring being illuminated by the sun would again be visible from the earth; but as the earth on the 21st of June must in its revolution be again on the other side, Saturn in his course falling in with the earth in the plane of the intersection of the ring and the ecliptic, will again pass through it on the 18th of August, at which time the ring will appear once more as a fine stripe of light. After that day the northern surface of the ring will be visible from the earth, and, as the line of the plane will then be without the earth's orbit, will, during the next fifteen years, remain always visible.

From the 7th January to the 21st of June, the southern surface of the ring being illuminated, the ring will be visible; but

but from the 21st of June to the 18th of August no part of it will be seen; as its darkened side during that period will be turned towards the earth. This disappearance and reappearance of Saturn's ring will be of importance to astronomers, as it will enable them to determine the direction of the plane of the ring, and will afford a curious spectacle to amateurs who are provided with good telescopes. The ring, which already appears as a broad stripe of light, will continually become smaller till the 15th of June; after which time it will disappear till the 15th of August, when it will appear as a fine stripe of light, which will increase in breadth till the aperture between it and the planet will increase during the first seven years. As Saturn till the month of June sets at midnight, the decrease of the ring may always be observed; but when he sets earlier it will be possible to see him only a short time in August, as he will be lost in the sun's rays.

Leyden,

March 10, 1803.

**XL. *Memoir on the Fossil Caout-Chouc, or Elastic Bitumen of Derbyshire.* By FAUJAS-SAINT-FOND\*.**

**A**BOUT twenty years ago some insulated portions of a kind of blackish bitumen, compressible and even elastic, which had some sort of resemblance to old leather, were found in the natural fissures of one of the mines of Castleton. The young miners having set fire to some of this matter, it burnt with a bright flame, emitting an odour which did not appear to be disagreeable.

At that period mineralogy was not so well known as at present in England, and particularly in Derbyshire: no one paid attention to this fossil, or endeavoured to ascertain its nature.

Twelve years after, a heavy storm of rain having produced a deep ravine on the side of one of the hills which surround the village of Castleton, a similar kind of bitumen was found between the fossil strata of argillaceous schist which exists at the bottom of that hill.

More care was then taken to collect this matter, some of which was sent to Derby, Edinburgh, and London; and so great a relation was found between it and the *caout-chouc*, known under the vulgar name of *elastic gum*, that mineralogists did not hesitate to consider it as the same as that brought

\* From *Journal du Museum National*, No. 4.

from Guiana and Peru. This opinion was soon confirmed by chemical analysis; and this singular fossil was the more sought after, and considered as a curious object of natural history, as it was more rare; and as it was thought an astonishing circumstance, that a substance which distils from exotic trees which grow only in the torrid zone should be found between strata of argillaceous schist in the bosom of the mountains in the northern part of England.

Lametherie mentioned this discovery in the *Journal de Physique*, without giving any details respecting the place where it was found: he only insisted on the analogy between this fossil bitumen and the caout-chouc.

At that period some varied fragments of this substance were sent to me, together with the matrices in which they had been found; and as I was acquainted with the places and the mountain where the discovery had been made, I mentioned this important fact every year in my geological lectures at the Musæum, when I treated of the exotic wood and plants found in a fossil state in countries absolutely opposite to those where these plants and trees grow at present: but I published nothing on the subject, as has been observed by Mr. Mawe, proprietor of the principal mines of sparry fluor of the neighbourhood of Castleton, and author of *The Mineralogy of Derbyshire*, lately published; a work which will be exceedingly useful to naturalists who intend to visit that county, so abundant in a variety of mineral productions\*.

Mr. Mawe, when last at Paris, brought me some very remarkable specimens which were wanting in my collection of fossil caout-chouc; and it is my duty to acknowledge here my gratitude to him. He was so kind as to give me also some information on this subject; and he showed me at the same time a piece of fossil caout-chouc, discovered not long ago, consisting of an oblong mass two feet and a half in circumference and two inches and a half in thickness, weighing about three pounds. It is soft, exceedingly compressible, and of a black and somewhat olive colour; solid, hard, and shining fragments of a bitumen nearly similar are enveloped by, and adhere as it were to, the elastic mass, both in the inside and on the exterior faces. They are not larger than a common almond; some of them are *black like jet*, others of a *brown liver* colour. They may be easily detached from the compressible caout-chouc where these hard fragments are not abundant.

\* The Mineralogy of Derbyshire, with a Description of the most interesting Mines in the North of England, in Scotland and Wales. By John Mawe. London 1802. 8vo. one volume, with plates.

Mr. Mawe has made known in his *Mineralogy of Derbyshire* the principal varieties of the fossil caout-chouc; but as he has attended rather to those select specimens fittest for ornamenting collections, than to a systematic description of these pieces as well as of the matrices which accompany them, and as he thought it of no use in his plan to enter into details respecting the nature and depth of the places where the fossil caout-chouc is found, I shall supply the deficiency in this part, which is so intimately connected with geology.

In order to give a topographical idea of the place, I shall observe, that in going from Derby to Castleton you are obliged to ascend a pretty rapid acclivity to a large plain in the form of a mountain, which extends for several leagues in every direction. All this elevated, rugged, and rocky district is known by the name of High Peak. It is in general calcareous, and even abundant in shells, except some argillaceous strata, and particularly several veins of trapp which intersect transversely the calcareous strata, or which, disposed sometimes in banks, proceed nearly parallel with beds of limestone. I shall say nothing of the veins of lead ore, of the beautiful crystals of sparry fluor, of the calamine and other minerals found in this district. For an account of these I shall refer to the *Description of Derbyshire*, Whitehurst on the *Formation of the Earth*\*, and Ferber's *Oryctographia of Derbyshire*†, or that which I gave myself of the environs of Buxton and of Castleton in my *Tour through England and Derbyshire*‡.

Castleton is not seen till you are, as I may say, above it, and till you arrive at the edge of a rapid and steep declivity, which intersects, in an abrupt and sudden manner, this part of the mountain surrounding the basin, at the bottom of which the small village in question is situated as in the middle of a funnel.

The revolution which divided this mountain, by tearing asunder its sides, arose, no doubt, from one of those grand natural events similar traces of which are to be found even in the High Alps; but here this terrible commotion, by opening the bosom of the earth, has uncovered several mineral riches which without this accidental circumstance would never have been known.

\* *Inquiry into the Original State and Formation of the Earth, &c.* by John Whitehurst. London, 4to. with plates.

† *Essay on the Oryctography of Derbyshire*, by Mr. Ferber, translated from the German.

‡ *Voyage en Angleterre, en Ecosse et aux Îles Hébrides, &c.* 2 vols. 8vo. avec fig.

One of the precipices of this mountain is known by the name of Hay Cliff: it is calcareous, and contains shells and other marine productions in a state of petrification. The other is called Mai-Tor: it is also calcareous, and towards the foot there are some galleries of a mine, on a vein which terminates near the surface, and of which the matrix is a milky calcareous spar containing large-grained galena, but in small quantity. There are found also towards the bottom of the same precipice, as well as in some other parts, fissile strata of argillaceous schist, very often marly, which imbibe water in the time of rain, split, and occasion the solid and stony masses which rest above them to crumble down\*.

Some of the schistous strata in question have a certain hardness; but all in general have a character of alluvion which an experienced eye cannot mistake, and yet their formation is of the highest antiquity. It is at a depth which, without fear of being deceived, may be estimated at least at four hundred and fifty feet below the upper stratum, that the fossil caout-chouc has been found, and particularly the large fragment in the possession of Mr. Mawe. In general it is in small cavities, in a kind of nests between the fissile strata, that this bituminous matter is contained; and as in this state it is sheltered from the action of the air, it needs excite no surprise that it should have experienced only very little alteration, and that its nature is not changed.

In the last place, one of the cavities, twenty-two inches in length and five inches in height, contained a great deal of caout-chouc. By these means very fine specimens of it may be obtained for collections, at a very moderate price; and it is to Mr. Mawe that we are indebted for this obligation. This gentleman, who with his father-in-law Mr. Brawn possesses a large manufactory of sparry fluor, of which they make elegant vases and other ornaments, carries on trade in a very honourable manner with the minerals of Scotland, Derbyshire and other parts of England, a depôt of which he proposes to establish at Paris, which must, no doubt, be of advantage to the progress of mineralogy.

I divide the fossil caout-chouc of Derbyshire into elastic or compressible bitumen, and solid, hard, and brittle bitumen.

\* "The earth and stones which crumble down from this mountain," says Ferber, who visited these places several years before I did, "form in several places small hills, the size of which daily increases, and which the people consider as one of the seven wonders of the Peak."—*Essai d'Ortogographie du Derbyshire*, par Ferber, Trad. Francoise, p. 21.

# SECTION I.

## *Compressible Bitumen.*

*Variety I.* Elastic bitumen of a blackish brown and somewhat olive colour, soft, exceedingly compressible, unctuous and somewhat fat, of a slightly aromatic smell, but having a little of the insipid odour of natural caout-chouc; inflammable, and burning with a bright brilliant flame, leaving a black oily residuum which does not become dry. The fragments of this variety are sometimes an inch and a half in thickness and four inches in breadth. They are the largest except the fragment in possession of Mr. Mawe, which is considerably larger.

*Variety II.* Fossil caout-chouc, split and cracked in the whole exterior surface, which was in contact with the matrix from which it has been detached. It is dry in that part, but nevertheless compressible; black on the surface exposed to the contact of the air; but remarkable in this respect, that when a lamina of about a line in thickness is cut from this caout-chouc with a very sharp knife, it appears in the inside of a yellowish white colour: when cut in a very thick part, where the action of the air has not been able to render it black, like certain balls of the elastic gum brought sometimes from Para, it has the same appearance as the usual caout-chouc. Another circumstance worthy of attention is, that in the part recently cut a kind of acid matter is observed to ooze out, which is not of a disagreeable taste, and which appears to be of the nature of the pyrolignic acid. This caout-chouc is pellucid on the edges, and almost of a hyacinth red colour.

Scarcely has the air exercised its action on the part recently cut, which is white, when it assumes a slight reddish tint, which daily becomes darker, and acquires a dark mahogany and then a blackish brown colour; at length, at the end of a month, if exposed to the air, it becomes of a deep black.

*Variety III.* The same caout-chouc as the preceding, but of a somewhat firmer texture and a ligneous appearance. When viewed with a magnifying glass, it is observed that the fibrous undulations seen in this variety are merely the effect of the milky substance which flowed from the tree more or less slowly or at different times, and which did not acquire consistence till it had lost the mucilaginous water which held the matter of caout-chouc suspended or in solution. I particularly mention this fact, to show that this variety has no real relation to the wood of the tree which produced at that period the elastic bitumen.

*Variety IV.* Fossil caout-chouc, compressible, and having a relation to the first variety, but of a darker colour, adhering to gray calcareous spar mixed with some grains of galena.

*Variety V.* Caout-chouc of a liver brown colour, much less unctuous to the touch than the first variety, but compressible, and having the aspect of the real natural elastic gum; and the more remarkable, as it is found to become solid, as we may say, in some parts, where it acquires a much greater hardness, and even becomes brittle, and acquires a vitreous splendour. This gradual change is so striking, that we cannot consider this hard bitumen, which in the present specimen is of a yellowish colour, as a hard bituminous matter accidentally enveloped by caout-chouc.

## SECTION II.

### *Solid and Brittle Fossil Caout-Chouc.*

No. VI. Black solid caout-chouc, hard and brittle like jet, exceedingly brilliant on its fracture, which is conchoid, and sometimes finely marked with striæ disposed in the form of rays, proceeding from the point of fracture, and expanding in the manner of a fan; electric by friction, opaque throughout the whole mass, but pellucid at the edges, and particularly when viewed in a strong light: its colour is then a red, almost as bright and agreeable as that of the hyacinth, and analogous to the colour observed on the edges of the elastic caout-chouc of the second variety, seen in the light which seems to announce their identity.

No. VII. Another variety of hard caout-chouc, similar to that of No. VI. in hardness and splendour, but which is of a liver brown colour. It is entirely pellucid by the light of a lamp even throughout its whole substance, though opaque in common day-light, and its colour is then similar to that of the hyacinth.

No. VIII. The same variety as the above, but of a paler liver colour. It has besides the same characters and the same properties as the other hard and brittle kinds of caout-chouc; but it is remarkable for still adhering to its matrix, consisting of semi-transparent milky calcareous spar, with thick brilliant laminae of galena. The caout-chouc here not only adheres to the calcareous spar but to sulphurated lead, and is intimately mixed with both.

Mr. Mawe, in his *Mineralogy of Derbyshire*, mentions the following very curious fact:—"A variety, the only one I possess of the kind, is elastic bitumen in a petrified marine shell inclosed in the rock;" also "another specimen no less rare

rare is obscure caout-chouc, but transparent in the light, inclosed in crystallized fluor." Fossil caout-chouc, according to Mr. Mawe, is found also in sulphated barytes.

If it be considered that trees and other vegetables, which produce natural caout-chouc in such abundance that the matter can flow down and accumulate at the root of them when the wind or any other accident lacerates the bark or breaks the branches, are all exotic, it will be allowed that this is a curious geological fact, which coincides with that of fossil amber, which has been found, and is still found, in some coal mines, and in turf soil of an antique origin, and which differs from that formed in the common turf mosses. We are not yet acquainted with any vegetable productions which furnish caout-chouc in abundance, but the following :

1st, The *vabea*, a species of apocinea, which grows in Madagascar, and of which Lamarck has given a figure in his *Illustrations de Botanique*.

2d, The *urceola elastica* of Sumatra and of Pullo-Pinang, discovered by Mr. Howison, an English surgeon at Pullo-Pinang, and described in the fifth volume of the *Asiatic Researches* by Dr. Roxburgh \*. This plant is of the family of the apocineæ.

3d, The *bevea Guianensis*, described and exhibited in a figure by Aublet in his *Plantes de la Guiane*, is a large tree of the family of the euphorbia. It rises to the height of more than forty feet; its trunk is sometimes above two feet in diameter; and the natives of Para make bottles, boots, and other articles of the caout-chouc which distils from it. It is the same as that mentioned by M. de la Condamine in the Memoirs of the Academy of Sciences for 1736, which grows also in the province of Esmeralda, in Peru, which the Maina Indians call *caout-chouc*, and of which they make bottles by means of earthen moulds: they use it also for torches to give them light.

4th, The *artocarpus integrifolia* of South America, a tree which approaches near to the mulberry and fig-tree.

5th, The *ficus religiosa*.

6th, The *ficus Indica*.

7th, The *bippomane biglandulosa* (manchineal tree).

8th, The *cecropia peltata*: the two last among the euphorbia produce also a milky juice analogous to the caout-chouc.

\* See Philosophical Magazine, vol. vi.

XLI. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them.* By C. LEFEBVRE, Member of the Council of Mines, of the Philomatic Society, &c. &c.

[Continued from p. 164.]

*Department of La Côte d'Or.*

SOME indications of coal have been announced here, and particularly in the communes of Aresne, Turcey, Montbard, and Chevauney. They deserve to be examined; and Champeaux, engineer of mines, now employed in that district, will no doubt give some accurate information on the subject: but hitherto no coal mines have been worked in this department; it can receive its coals from Blanzi by the canal of Charollois, and by going up the Saône as far as Saint-Jean-de-Lône, where the canal of Burgundy begins.

*Department of the Côtes-du-Nord.*

No coals have yet been found in this department. Indications of coal have been announced near Lannion and Quimper-Gaezence, in the neighbourhood of Pontrieux; but no attention has been paid to them. It is supplied therefore only by means of its sea-ports, which may receive the coal brought from the mines of Litry, in the Calvados, and those which abound in the northern departments, which may be conveyed by land to the canals which terminate at the sea.

*Department of La Creuse.*

Several coal mines (14) are worked in this country, which is still little known in a mineralogical point of view, and which appears worthy of being carefully examined. The communes where coal mines in a state of working are found, are those of Couchezotte, Bosmoraud, Vavory, St. Palais, and Fautmazuras.

Though their production, according to the information obtained by the Council of Mines, is estimated in general at 126,000 myriagrammes, it certainly exceeds that quantity, because several of the mines have not yet been worked with proper activity. The mean price of the coals at the pit is 10 cents per myriagramme.

These mines are not much worked, because there are very few means of consumption.

If the Creuse, which is said to be navigable as far as Guéret, could be rendered navigable for boats to Ahun, it would

would open a very useful circulation for the coals of the mines in the neighbourhood of that commune, and would facilitate the transportation of them as far as the Vienne, to which it joins in the department of Indre-et-Loire. These mines, then, would supply in part the consumption of the department of Indre.

On the other hand, if it were possible to render the Thirion, which passes Bourgneuf, navigable from that town to its junction with the Vienne above Limoges, and to ensure the navigation of the Vienne from Limoges to Châtelleraux, where it begins to bear boats, an extensive consumption would be opened for the coals which are on the north and south of Bourgneuf, and make this fuel circulate in the departments of La Haute-Vienne and of La Vienne. This circulation would be exceedingly useful, and increase manufactories and industry.

*Department of La Dyle.*

This department possesses no coal mines; but it borders on that of Jemappes, the numerous mines of which supply more coals than it has occasion for. It is of importance, therefore, that this fuel, the use of which is general in that country, may be obtained at a price suited to the different places where it is consumed; and that the roads be repaired and carefully maintained in a proper state. Without this attention, the dearth of carriage would throw the inhabitants of La Dyle into a deplorable state of distress in regard to fuel, and produce a hurtful stagnation in the mines of the department of Jemappes.

*Department of La Dordogne.*

The cantons of Cransac and Teraillon (15) afford collections and strata of coals of a good quality and exceedingly rich. These mines would be an object worthy being worked with activity, if the navigation of the Vefere were rendered more certain, and easier.

At present they are worked only at the surface by some proprietors of land, and supply only the local consumption. The product of these coal mines is not known; but it is certain that the working of them would be easy, and that they are capable of affording great resources for a long time.

*Department of Doubs.*

Several indications of coals have been announced in this department (16). Researches even have been made in different parts, but hitherto no coals have been dug up.

According

According to the general measures which have been taken by the government, this department is one of those where an engineer of mines will be employed; and we have reason to hope that researches will be made, and followed with success.

A very considerable mass of fossil bituminous wood has been discovered at Grand-Denis, in the neighbourhood of Ornans, in the commune of Flanchebouché. This substance has not entirely the same qualities as coals; but in the state in which it is found at Grand-Denis it may be advantageously employed for several purposes, and particularly under boilers. The managers of the salt-works, who have caused trials to be made with it, are going to employ it for evaporating the water of the salt-work of Montmorot. This will produce a great saving in the wood employed in that manufactory.

#### *Department of La Drôme.*

The discovery of coal mines in this department (17) has often been announced; but, according to the reports of the engineers of mines who have visited them, it appears that these indications were nothing else than bituminous fossil wood, which is frequently found in strata of sand, and particularly in the environs of Crest in the district of that name, and in the territory of several communes in the environs of Nions.

This fossil wood has been dug up, and particularly at Crest; and though it cannot be applied to the same purposes as coals, it is still useful in these countries in the manufactories for spinning silk.

The department of La Drôme may receive abundance of coals, at least for supplying the communes which lie near to the banks of the Rhône, as those of the departments of Ardèche and La Loire can be conveyed to them on that river.

#### *Department of the Scheldt.*

The mines of the departments of the North and of Jemappes furnish abundance of coals to the inhabitants of this department, in which no coal mines are worked.

#### *Department of Eure.*

In this department no coal mines are known. It cannot procure this fuel but by going down the Seine to its mouth, or unless some arrive by sea at Honfleur. This port may be supplied from the coal mines of Litry, in Calvados, or from those of the departments of the North.

*Department*

*Department of Eure-et-Loire.*

As this department, like the preceding, has no mines in a state of being worked, it can procure no coals but those conveyed down the Loire as far as Orleans, which supply its southern part, and those circulated on the Seine, which supply the northern. But the land carriage there must render this fuel very dear.

*Department of Finisterre.*

There are no coal mines in this department (18) which can be considered in a state of productive working: however, in consequence of old indications, some researches have been within these few years resumed in the neighbourhood of Quimper.

These researches have afforded some hopes. Small veins of coal have even been found; and as a mine of this kind would be of great importance to the port of Brest and the other sea ports and arsenals of this department, the minister of the marine has caused these researches to be continued near Quimper with great activity, according to plans approved by the Council of Mines.

Several other indications have been announced at Cleden and at the bottom of the Bay of Dinan. C. Berth had given in the year 6 some information in regard to this district; in consequence of which he was allowed to continue the researches he had begun, and to put himself in a condition to obtain a grant when his discovery should be fully established. C. Berth set out for Egypt, and it does not appear that attention has since been paid to his labours: a coal mine in this point would be of the more importance, as it would be within reach of the harbour of Brest.

*Department of Forêts.*

No coal mines are worked in this department, which may receive in the southern part the product of the abundant mines in the environs of Saarbruck, and the coals which may be conveyed on the Moselle.

*Department of Gard.*

This is one of the departments of the south of France (19) where this kind of fuel is most abundant. On the north of Alais the mines of Cendras, of Portes, of the Forest of Abillon, La Grand-Combe, and Pradel, furnish about 2,200,000 myriagrammes of coals per annum.

The

The coal mines of Banes, Robillac, Meranes, Saint-Jean-de-Valerisque, furnish at least 900,000 myriagrammes.

Several strata of coal are still worked in the environs of Pont-Saint-Esprit, and towards Laudun; but it is inferior in quality to the before-mentioned. This is the more unfortunate, as the situation of these mines on the banks of the Rhone would render them of great importance on account of the facility with which they could be transported.

If the coal mines in the environs of Alais possessed the same advantages in regard to transportation, the quantity dug up might be increased to ten times the present quantity, without any fear of the mines being exhausted in many years. But they cannot be exported from the country, in consequence of the dearth of carriage.

The mean price of the large coals at these mines is 7 cents per myriagramme, and that of the small coals from 4 to 5 cents.

It appears that a canal is now constructing from Nimes to Saint-Gilles. The mineral riches and abundant resources in fuel, which the mines in the neighbourhood of Alais would furnish for ages, are of so much importance, that a communication ought to be opened with that country by means of a counter-canal to supply the place of the bed of the Gardon, which is not navigable.

In this department there are also some strata of coal worked in the environs of the commune of Vigan. At present the working is not carried on with much activity; some of the pits have even been abandoned in consequence of law-suits, which are on the point of being decided.

The common amount of the product of this canton may be estimated at least at 200,000 myriagrammes.

These mines cannot find much consumption except by the Herault; but this river is not navigable in the neighbourhood of Vigan.

#### *Department of La Haute-Garonne.*

In this department there are no coal mines worked. Some soundings have been made in the environs of Toulouse, in consequence of some supposed indications. These soundings have not confirmed the hopes which had been conceived.

Fossil wood in the state of very fine jet has been found in the Forest of Montbrun toward Montesquieu and Rieux. Specimens of it may be seen in the collection of the Council of Mines.

The department of La Haute-Garonne may be supplied with

with coals in the eastern part by the mines of Carmeaux, in the department of Tarn. The products of these mines are embarked on the Tarn, and enter with that river into the northern part of the department of La Haute-Garonne. But the difficulties which occur in the navigation of the Tarn greatly increase the price of the coals of Carmeaux. This inconvenience might be removed without much expense. We must therefore hope that measures will be pursued to remove these obstacles, which are equally prejudicial to industry, to commerce, and to all the consumers of these departments. The coals destined for Toulouse and the southern part of the department are deposited at Port Saint-Sulpice, from which they are transported by land carriage.

*Department of Gers.*

This department, in which no coal mines are known, can obtain this fuel only by land carriage from the entrepôts of Toulouse, or from some of the ports of La Garonne in its northern part.

*Department of La Gironde.*

Collections of bituminous fossil wood deposited in strata of sand have been discovered in several places in the neighbourhood of Bourdeaux. As this fossil wood seems to be dispersed in great abundance, it would be of advantage to pursue the researches which have been made, with more constancy than has hitherto been the case.

These researches might not conduct to the discovery of a coal mine; but the fossil wood might be used with great advantage as fuel in different operations for which wood is employed.

There are no coal mines worked in this department, but it receives by the Garonne the coals of Carmeaux. This fuel might also be procured with more ease from the abundant coal mines which are known between Teraillon and Bergerac, on the banks of the Vefere and the Dordogne, if the navigation of these rivers were rendered easier.

This department might receive also by the mouth of the Gironde the products of the rich mines of the departments of the North. An exchange of the coals, iron works, and of a variety of other articles manufactured in these departments, for the wines and brandy of the Bordelais, which are in request in the whole North, might be made by the French naval trade as well as the English. It is even probable that we should obtain great advantages for the ironmongery trade in India, in consequence of the low price at which the manufactories

manufactories of the north of France could furnish these articles, of which Bourdeaux would be the entrepôt either for America or for the East Indies.

*Department of Herault.*

Coal mines are frequently met with in this department (20). The canton of Bedarieux affords some immensely rich, such as those of Saint-Gervais, of Camplong, of Bouffaque, and Graissefac; towards the south, in the canton of Roujan, those of Boufquet, in the commune of Neffies; towards the south-west, in the canton of Saint-Chiniau, the mines of Cessenon; and towards the south, near the canal of the two seas, those of Azillanet. Some have been found also in different places in the environs of Montpellier. A lease is about to be granted for working those of Saint-Gely-du-Fesq.

Though the greater part of the coals procured from these mines is not of the best quality, they are a great relief, in consequence of the dearth of wood, and of the numerous manufactories in which they are employed.

But in general, the means of circulation in the interior of this department are neither numerous nor easy. The coals of the mines in the neighbourhood of Bedarieux, which might furnish a great many, cannot conveniently be transported. Some individuals are soliciting for grants subject to the condition of making a road, which would soon give more importance to these mines by facilitating the carriage of them to Bedarieux. It would be necessary also to have a communication with the canal of the two seas, less expensive than that by land. It appears that the river Ourbe might easily be rendered navigable from Bedarieux to Beziers. It would then answer very well the proposed object.

The price of coals at these mines is 15 cents per myriagramme. But this price is doubled when they have been transported as far as the canal.

The coal mines which are nearest, such as those of Azillanet, are attended with some advantages in this respect.

The whole product of the different mines of this department may be estimated at 1,800,000 myriagrammes.

In consequence of the superior quality of the coals of Carmaux, in the department of Tarn, they are received by the consumers along the canal, in preference to those already mentioned, though the price is nearly double.

*Department of Jemappes.*

A very large portion of this department (21), and particularly

cularly its southern part, may be considered as an immense mass of coals, in some places scarcely covered by strata more modern than the dépôts of that mineral.

The geologue, in traversing the interior of these mines, is astonished at the varied phænomena exhibited by the numerous and successive strata of coals, the inflection, curvatures, or turns of which, in an inverse direction and parallelism, open a vast but difficult field for conjectures respecting their formation, and the revolutions of the globe which must have produced these results; but to enter here into a consideration of these wonderful effects, a description of which must be reserved for particular treatises on the operations of nature, would lead me too far from my present object.

I shall therefore content myself with giving some idea of the resources which this country furnishes, not only to its inhabitants, but to those of several other departments; by which it will be seen how much its productions in coal might be increased, and that they would be sufficient to supply for many years all those parts of France which receive coals by sea from foreign countries.

More than 3000 pits are worked in the environs of Jemappes, Mons, and Charleroi. They are far from being carried to their maximum of activity. The sum of their products, however, amounts, at the least, to 220,000,000 myriagrammes annually; and this product might easily be doubled, if the consumption increased in the same proportion.

In regard to the quality of these coals, it is exceedingly various: these numerous mines furnish coals of every kind, and the price at the pits differs according to the quality from 5 to 9 cents per myriagramme.

The places where they are consumed are, the departments of the Sambre and Meuse, La Dyle, the Scheldt, the Deux-Nethes, and Batavia, in competition with the English coals.

The means of circulation are the river Haine, the Scheldt, the canals which communicate with these rivers, the Sambre and the Meuse, and the large roads from Mons and Charleroi to Brussels.

It may be readily conceived of how much importance it would be to this department, which employs a great number of hands in working its coals, and also to those countries to which these coals might be transported, if the canals and roads were kept in a state of better repair.

Every degradation of the roads which retards conveyance, or which renders more horses necessary, multiplies expense and increases the price of the coals; which ought to be kept

as low as possible, both on account of their utility in our manufactories, and of the necessity of maintaining a competition with the English coals.

The carriage by the causeways from Charleroi and Mons to Brussels adds greatly to the price of these coals. A canal has been projected for uniting the Sambre towards Thuin with the small river of Senne; which would convey the coals of the mines in the neighbourhood of Charleroi to Brussels, and would thus form a communication with the Scheldt and Batavia, so as to prevent the necessity of land-carriage.

This canal, valuable in this point of view, would be of utility also to the export trade of these countries in regard to the glass and nails sent to Holland and to our own ports, and which are thence conveyed to the Indies.

It would be of utility also to government for transporting the timber of the Forest of Souane, through which it would pass.

To point out these improvements will, no doubt, be sufficient to induce government to pay serious attention to them; but until new means of circulation be provided, it is of importance that the existing roads should be properly repaired.

I shall terminate this article by observing, that it is the interest of France to facilitate its commercial intercourse with Holland, and particularly that which relates to the importation of our coals into that country: consequently, instead of laying a duty on their exportation to the amount, as is said, of 20 per cent., this branch of commerce ought to be entirely free. It would be good policy also to encourage by bounties the exportation of the coals of this department, especially when destined for French ports in parts of our territory which cannot receive this fuel from the mines of the interior, and which if not supplied by our mines would procure them from foreign countries.

[To be continued.]

XLII. *On a new Kind of Painting with the Serum or watery Part of the Blood.* By F. CARBONEL, M.D. of Barcelona\*.

THE advantages and utility of painting have at all times excited the emulation of professors and of artists. The progress of this art, which imitates nature, has fortunately corresponded to their wishes; but its advancement would cer-

\* From the *Journal de Physique*, Ventose, an. 11.

certainly have been more rapid if artists had been sooner made acquainted with the new chemistry. This assertion is proved by the present discovery.

The necessity of painting surfaces of different kinds, their various situations, and the complex circumstances which attend the application of colours, have given rise to the different kinds of painting which are known; such as painting in fresco, painting in size, oil-painting, painting with milk, painting on porcelain, painting with wax, painting on glass, &c.

The journey of the king of Spain to Barcelona, and the preparation of the edifices destined for the use of their majesties, afforded me an opportunity of solving a new problem in regard to this art. It was necessary to discover some kind of painting of the common colour of stone which might be able to resist rain, which should be unalterable in the sun, easy to be applied on wood, and which should dry soon and exhale no bad odour; in a word, a painting equally durable with that in size, and which should have the property of being able to withstand the inclemency of the weather. The solubility of gelatin in water, the bad smell of oil, the weak adhesion of milk of lime to wood, and its ready alteration by heat, made me sensible that it would be impossible to have recourse to any of the known vehicles for securing the colouring matters. After some examination I found that albumen\* was the only substance which could answer the required conditions, on account of its insolubility in water, and the consistence it acquires by combining with calcareous earths. This substance, prepared by the vital power in the animal organs, is found almost pure in the white of an egg or albumen, and is found in combination in different animal substances. But the great viscosity of the white of an egg, the clots which are formed by its combination with lime, and particularly its dearness, made me give up all idea of employing it for painting a whole edifice.

I then conceived that the serum of blood, the principal part of which consists of albumen, would be attended with all the advantages of white of egg for the proposed end, without having any of its inconveniences. Chemical analysis of animal substances, indeed, shows that the serum of blood, commonly called the water of the blood, or, improperly, the lymph of the blood, is a kind of animal mucilage composed of albuminous or gelatinous substances in different propor-

\* This painting was much wanted for the bridge which joins the *hotel de la Douane* to the palace of the captain-general, as no other kind could be employed for fear of injuring the queen's health.

tions, dissolved in an indeterminate quantity of water, constantly united to pure soda, which reduces the albuminous substance to an almost saponaceous state. There is found also in the serum phosphate of soda, of lime, and of ammonia. But these saline substances are not essential to this combination, since they not only may vary in their proportions, but be wanting either in whole or in part; and even unite in the serum to other salts, such as the muriate of potash, the phosphate of magnesia, &c., without the albuminous, alkaline, or gelatinous substance of the serum being changed, or exhibiting different characteristic properties.

This doctrine, received by all the celebrated modern chemists, induced me to think that the albuminous substance of serum, modified by its combination with gelatin and soda, ought to form with calcareous earth a composition which would retain no more of that viscosity and tenacity assumed by the mixture of the white of an egg and lime, than what might be necessary for answering the proposed end.

These reasons, and the low price of this liquid in large cities, soon induced me to try the effects of the serum of blood with pulverized quicklime; and I found, with much satisfaction, that my first trials corresponded in every respect to the hopes I had conceived.

*Practice and Application of the new Kind of Painting.*

The preparation of this new kind of paint is easy, and requires very little time. Nothing is necessary but to pulverize the lime, and to dilute it in the serum of blood until it forms a liquid of sufficient thickness for painting; which is then to be applied in the usual manner. The colour which this composition acquires is whitish, and more or less shining, according to the purity of the serum and the whiteness of the lime. This earth may be employed either in the state of quick or caustic lime reduced to powder, or well flaked with a small quantity of water to lessen the adhesion of its integrant parts. It must then be reduced to a fine powder, and be sifted through a close sieve. If kept for some days, it is necessary to preserve it in a close tub or earthen vessel, to prevent its combination with the carbonic acid and water of the atmosphere. The serum is separated from the curdled blood by decantation: but care must be taken to shake the matter as little as possible, and to suffer it each time to remain at rest.

In this composition two things only are to be observed: the first is, that serum being a fluid highly susceptible of corruption, it must be employed the same day it is separated; or  
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at furthest the day following, provided it be kept in a cool place sheltered from the contact of the air, especially during the summer, when it alters much more easily.

In case of doubt, inspection of the fluid alone may serve to determine its quality; for the signs of its fermentation are very evident. This observation is applicable also to the colouring liquor, but in a less degree. It follows from this circumstance, that the vessels which have been used for containing, preparing, or applying paint made with serum ought to be washed every day. The second precaution to be taken is, that as the re-action of the serum and lime increases in the course of time, as well as the density of the mixture, in order that it may be preserved in the necessary state of fluidity, or to diminish or increase it according to the nature of the substances to be painted and the number of strata to be applied, another vessel filled with serum must be provided, to give to the colouring liquor the necessary fluidity. It thence follows that the mixture must be prepared as it is used.

Having assured myself of the successful and constant effects of this kind of painting, I tried to mix it with different colouring substances, to render the application of the invention more general.

The well known re-action of animal substances and of lime on the metallic oxides, and particularly on those in which oxygen is united to the metal with little force, made me suspect an alteration of some colours when mixed with this liquid; an alteration naturally produced by the separation of a part of the oxygen from the metallic oxides. On the other hand, the analogy which coloured chalks have with calcareous earth induced me to believe that their combination would be attended with the happiest result. The oxides of lead, copper, and arsenic, indeed, undergo a singular alteration in their respective colours, when they are united with serum and calcareous earth: but this is not the case with cobalt green; which on that account appears to me to be very proper for being used as a colouring matter in this new kind of painting. Those earths, however, called red, green, black, or yellow, which are of an argillo-calcareous nature, when mixed with these substances, may furnish different agreeable colours of a durability equal to the blueish colour produced merely by the mixture of lime and of serum.

I have however remarked, that the durability of this painting is considerably altered when there is mixed with it a very large quantity of colouring earth, such as the black earth used for painting an iron colour, or the representation of white marble with black spots. To remedy this inconvenience, no-

thing will be necessary but to mix a little white of an egg with the serum before it is united with the lime or the black earth. I must here add, that wood painted in this manner may be polished, and made to acquire a certain degree of lustre. In this case I have observed that fresh spermaceti may be substituted with advantage for the tallow or oil in which the rags employed for rubbing wood before it is polished are commonly dipped. This kind of paint may be applied also with the same success to earthen ware, lead, iron, cut stone, &c.

The circumstances which induced me to employ this new kind of painting, obliged me to make an extensive use of it, without allowing me time to carry my operations any further. More than 300 arrobes\* of serum were consumed in the course of a few days. In this manner we were able to paint not only the passage and bridge of the king's palace, but also the windows and doors of the same edifice, those of the magnificent building of the Exchange, the *Place de Taureau*, and many private houses. The happy effects it produced, when exposed to the eyes of the public, are a sufficient proof that practice here perfectly corresponds with theory.

It is hardly necessary to observe, that the surfaces to be painted must be smooth; that the wood must be covered with a stratum of plaster; and that this painting must not be applied over painting with oil or with turpentine, because the heat would make it crack.

I propose to continue the small number of trials I have already made, to render the use of my discovery more general. The following is the result of those which I have hitherto been able to attempt.

In consequence of the viscosity, adhesion, and speedy desiccation of this composition, it may be employed in the same manner as bitumen, to cement with great solidity broken articles of every kind. In this case it opposes a strong resistance to the action of fire, water, many of the gases, and to vapour.

As serum contains a large quantity of albuminous matter, having myself separated from it nearly half its weight, it may be employed with advantage for clarifying sugar, wine, and other substances, in a more æconomical manner than by the white of an egg.

This new composition may be employed also for covering the surface of damp walls intended to be painted in oil colours; taking care, however, to cover it with a coating of

\* An arrobe weighs 25 or 26 pounds.

plaster and glue, to prevent its re-action on the colouring parts of the painting.

The hardness of this substance, and its susceptibility of receiving a polish, induce me to believe that it might be employed on stucco, and for imitating it on walls and ceilings.

It may be used also for uniting water-conduits, and for cementing stones of all dimensions, and especially those which are put into wood caissons for building under water.

It appears from different works, that the ancients had conceived the idea of employing blood in the composition of their cements and mortar; but the use of it in painting was before unknown. Besides, there was too great a distance between this use of blood and that of serum freed from the fibrous part and the colouring matter, to admit of any doubt that the principle of my discovery is entirely my own.

It follows from what has been here said, that this new kind of painting may be added to those already known, and that it may be called painting *in serum*; that it is as easily applied as prepared; that the elements of it are common, and of little value; and that it unites in it all the advantages of painting in oil and in distemper, without having any of their inconveniences.

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**XLIII.** *On the Kind of Air-Furnaces employed in Iron Foundries for casting large Pieces of Ordnance, Shafts for Mills, Cylinders, and other heavy Articles.* By Mr. DAVID MUSHET, of the Calder Iron Works\*.

THE furnaces about to be described are employed for melting pig-iron with the flame of pit-coal. Furnaces of this kind are constructed of various sizes according to circumstances. The small sizes will run down from seven to ten hundred weight, and are used in small foundries for what the trade call *jobbing*.

Fig. 1. (Plate IV.) a ground plan of two large air-furnaces, and chimney for melting pig- or cast-iron with the flame of pit-coal.

The letters A B C D point out the exterior dimensions of the stalk or chimney, which is first erected, leaving two openings or arches into which the fore-part of the furnaces are afterwards built. The breadth of the chimney at the particular place which the plan exhibits is 16 feet from A to B, and from A to D or from B to C 6 feet 6 inches. The plan is drawn at that elevation where the flame enters the chimney

\* Communicated by the Author.

by the flue or throat, narrowed on purpose to throw back part of the flame, and keep the furnace equally hot throughout, as may be more particularly viewed in the vertical section, fig. 2.

EE, the furnace bars on which the coals rest, and where the combustion is maintained.

FF, openings called teasing-holes, by which the coals are introduced to repair the fire.

GG, fire-brick buildings called bridges. These are meant to concentrate the flame, that it may act as violently on the metal as possible. Upon the height of the bridge much depends in fusing the metal speedily, and with little loss. The height of this may be seen in the vertical section, fig. 2. G.

HH, the charging doors, by which the metal is introduced in the shape and state of pig-iron, lumps, scraps, &c. &c. The iron generally occupies the furnace across to I, called the back wall, and is never meant to approach the bridge nearer than the dotted line, lest the metal in melting should run back into the grates; in place of descending into the general reservoir or cavity below. The corners or notches, *b, b, b, b*, receive a stout cast-iron frame lined with fire-bricks. This is hung by means of a chain and pulley, and can be raised and depressed at pleasure. This frame is, properly speaking, the charging door, and is always carefully made air-tight by means of moistened sand.

KK, the flues or openings by which the flame enters the chimney. These are 15 inches by 10. On maintaining these openings of a proportionate size to the other parts depend in a great measure the powers and œconomy of the furnace.

LL, lading doors, by which ladles are introduced, in the case of small furnaces, to lift out the metal and distribute it to the various moulds.

MMMM, binding bolts to limit within proper bounds the expansion which takes place in the building when the furnace is highly heated.

Fig. 2. vertical section of one of the furnaces, and its appropriate stalk or chimney.

E, the grates.

F, the teasing-hole.

G, the bridge.

H, the charging door.

K, the flue or opening into the chimney.

L, the lading door.

M M, the binder or binding bolt.

N, the interior of the stalk or chimney, 30 inches square.

OO, the fire brick-work, 9 inches thick.

PP, space

PP, space of 2 inches for stuffing with sand.

QQ, common brick building.

RR, cast-iron lintels, over which are thrown double 9-inch arches, so that at any time the inferior building can be taken down to make repairs, without shaking or in the least injuring the chimney.

S. The dotted lines here are meant to represent what is called the tapping-hole. When a large piece of goods is to be cast, lifting the metal with ladles would be impracticable. A sharp-pointed bar is driven up this opening. The iron then flows freely out into a large basin of sand made for its reception. It is then conducted, by collateral channels, into the mould.

The space under the curved dotted line from G to L, by S, is filled with a mixture of sand and ashes. When the furnace is prepared to melt, the whole of the bottom receives a stratum of sharp clean sand about two inches thick. This is broken up at night, and fresh sand is substituted for it before the fire is kindled in the morning.

Fig. 3. is a horizontal section of the chimney or stalk, taken where the flues assume a perpendicular direction. The letters in this figure correspond to those in the vertical section, fig. 2. The height of the chimney ought not to be less than 45 feet: if 50 feet, the effect will be sooner and of course better produced.

#### *Remarks upon the Melting of Iron in these Furnaces.*

The effect wished to be produced in air-furnaces is the fusion of a certain portion of pig- or cast-iron for the purpose of being poured or run into moulds to form articles of almost every description.

The preparation previous to melting is as follows:—After the bottom of the furnace is laid, and smoothed with fresh sand, and all the openings made air-tight, the furnace-man introduces a kindling at the teasing-hole, accompanied with new pit-coal. In a few minutes a considerable volume of dark flame mixed with smoke is produced. The fire quickly gathers strength; more coal is introduced; and the furnace now becomes filled with a yellow-coloured flame. By continuing this operation for an hour, or an hour and a quarter, the furnace and flame will have become completely white; the latter steady, and at times apparently without motion. The furnace-man now judges the bottom to have been sufficiently hardened for receiving the pig-iron without any risk of sinking. The charging-door is now opened, and the pig-metal thrown carefully and regularly upon that part of the

bottom formerly described as being appropriated for its reception. The door is again closed and made air-tight, and the operation of firing continued with unremitting care and attention.

The time of melting depends entirely upon the quantity of metal introduced. The furnaces described above are capable of melting from 50 to 60 hundred weight of metal each, and when there is a moderate circulation of air they will perform this work in  $2\frac{1}{2}$  or 3 hours. In half an hour after the metal is introduced it assumes a blackish red colour. It then begins to brighten with every additional fire, and in about one hour appears white, and begins to lose shape, and resemble a wreath of snow.

An eye accustomed to such heats will now discern the metal beginning to drop, and run down the inclined plane in very beautiful streamlets resembling quicksilver. Eight or ten of these are visible at a time, and after proceeding half way down begin to form junctions with each other, and flow connected into the general cavity or reservoir. By-and-by this becomes filled, and literally forms a beautiful molten mirror, in which sometimes part of the interior furnace is reflected.

The furnace-man, by searching at the bridge with his fire-iron or teaser, judges when the metal is nearly all gone. Of this he is certain by looking up from the peep-hole of the lading-door. If the streamlets of the running metal have ceased, then the whole is melted, and ready for running out.

In the operation of melting, the three following circumstances ought to be particularly attended to: the thinness or hotness of the metal; the waste or loss sustained in melting; and the quantity of coals employed.

The first is of the utmost importance; as many articles in the foundry business require the metal in a state of the greatest division; otherwise they will be found imperfect when taken from the sand, and unfit for sale. The furnace-man, therefore, is always on the watch to replace the fire as it decays, and keep a large and sharp volume of flame constantly passing over the metal.

The waste or loss of real metal is also an object of great importance. This always bears a relation to the quality of the iron, the strength and cleanness of the coals, and the judgment and attention of the melter. Strong iron is found always more difficult to fuse; this necessarily exposes it for a long period in contact with the flame. The reverse happens with metal that is more fragile, and easier broken in the pig. The length of the exposure in fusing depends on this; and  
other

other circumstances being alike, the loss or waste of metal will also be in the same ratio.

There are, however, other facts not unworthy of notice. No. I. pig-iron, or richly carbonated metal, when run from an air-furnace, will be found in point of quality little better than No. II. or carbonated iron. This is owing to a quantity of its carbon being destroyed during the fusion. The loss in melting No. I. iron, therefore, chiefly consists of carbon; and the deficiency of metal ought never, with a clean bottom, to exceed 1 cwt. in 20.

Carbonated or No. II. iron also becomes deprived of a considerable portion of its carbonaceous mixture in fusion; and when run from the air-furnace is seldom better than No. III. metal. The loss sustained in melting may be averaged at  $7\frac{1}{2}$  per cent.

No. III. pig-iron is, after melting in an air-furnace, found whitish or mottled. It is seldom susceptible of the same nice degree of division as the superior qualities, and loses in fusion a much larger proportion of metal, seldom under 10 per cent. and frequently  $12\frac{1}{2}$  or 15.

The quantity of coals requisite to melt a given quantity of iron is various, as much depends upon the quality and fusibility of the metal. If the furnace goes one heat a day with No. I. or II. iron, the quantity of coals will be from 20 to 25 cwt. for a ton of iron. If two or three heats a day, or as many tons of iron are melted at one kindling, the proportion of coals will be nearly weight for weight of the iron melted when the coals are mixed with a fair proportion of small: with strong large splint coals, one ton of good pig-iron may be completely reduced with from 12 to 15 cwt. including the previous heating of the furnace.

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XLIV. *Observations on the Freezing of Water and the Nature of Snow: in a Letter from Professor DRIESSEN to Dr. VAN MARUM\*.*

AS the present season has afforded us an opportunity to make experiments on the freezing of water and the nature of snow, I flatter myself that it will give you some satisfaction to be made acquainted with the results I have obtained in regard to this important subject; and I give you full liberty to publish them, if you think they are likely to be of any utility.

\* From *Algemeene Konst en Letter-Bode*, No. 14, 1803.

At present, however, I shall give you only a short account of my experiments, because I hope, in the course of a few years, to collect materials for a more elaborate work on the freezing of water, and the state of the atmosphere in winter; having been induced to turn my attention to the latter object by the observations I had made, and which I mentioned to you three years ago, on the force with which a considerable quantity of vapour, especially during the time of strong frost, is disengaged in an æriform state from freezing water, and rises, as it were, with violence into the atmosphere; and on the increase of volume which the air thereby acquires even in close vessels, and which it retains during the strongest cold.

When I traverse the snow-clad fields during the coldest days of winter; when I consider the dry state of the air, and of every thing around me, arising, no doubt, from a strong and continued tendency of the aqueous particles, of which I neither see nor feel any thing, to rise into the higher regions of the atmosphere; and when in consequence of the serenity of the heavens I can distinguish very small objects at a great distance, I cannot help admiring the power of nature, as yet so little known, by which air so cold and so much condensed can be so intimately connected with water. This water, in my opinion, can be considered only as in an æriform state, as such combined with the atmosphere by a very strong affinity, and forming with it one and the same body: I must observe also, that this affinity seems to increase in the ratio of the cold.

The atmosphere being an elastic fluid in which caloric is combined by the strongest affinity with its bases, it by these means appears to be more capable of preserving by the power of this union, in the state of atmospheric vapour, the æriform water which has so weak an affinity for its specific caloric. The air thus condensed, and containing more aerial particles in the same space than in summer, attracts, on this account, the water with much greater force. The water becoming æriform during the process of freezing, and in that state entering into combination with the air, has no need of external heat to maintain it in that state of union. The æriform state of water, which in summer is maintained by the heat of the surrounding bodies, is in this season preserved by a state of greater affinity.

Is the water then, during this cold, serene, wintery atmosphere, in a more perfect state of union with the air? I answer in the affirmative; and therefore I can readily conceive why the mercury stands at such heights in the barometer during that season. This air also may be considered as a receptacle

ceptacle for a very large quantity of combined caloric, which by the decomposition of the atmosphere returns again, in a great measure, with the water, as sensible heat to the surface of the earth: in the phenomena of combustion, respiration, and other great operations in the œconomy of nature, I can see not only a decomposition of vital air, but that also of aëri-form water, which is so intimately connected with air. But as I dare not venture to proceed further in this path, for which I yet feel myself too weak, I shall return to the principal object of this paper.

I have repeated the experiments which I before communicated to you, as already observed, and with the same result: I entertained no doubt that the dilatation of freezing water, previously deprived of air, is to be ascribed, besides to crystallization, in particular to the aëri-form water, which during the freezing being carried off with the liberated caloric, deranges in its passage and separates the crystals from each other. I had entirely given up the idea of the existence of air in chemical union with water, or with its bases, which could not be separated by boiling, and which by freezing might again be liberated in an elastic state, till the treatise of my friend Dr. Uilken, written in March 1800, came into my hands.

The well-known merit of this philosopher, the modesty with which he communicates his ideas to the public, made me consider it as my duty to examine his objections with all due respect; but at the same time to prove to my pupils, if possible, the truth of my experiments.

According to my experiments, water can be perfectly freed from air. In such water no air-bubbles arise during the process of freezing, provided the air has no access to it. Its communication with the air would instantly make them appear.

For the sake of those who may be desirous to repeat my experiments, I shall describe an infallible method by which the smallest traces of air in water may be detected. None of the means commonly used for the purpose are sufficient when accuracy is required, as is the case in the present instance.

I pour the water destined for examination into an earthen vessel, communicate a blue tint to it by a sufficient quantity of solution of litmus, which I preserve ready, of a certain strength, for such purposes: I then fill with it a flask in such a manner, that the aperture of the flask is kept under water during the time of filling; taking care to shake the flask, that the smallest air-bubble may not remain in it. I then invert the flask in the water, and introduce into it, in the common manner,

manner, pure nitrous gas until 1-16th or 1-20th of it is filled. I then place the flask in a dish, and intercept its communication with the atmosphere by means of mercury. Now, if there be any air in the water, its oxygen gas must unite with the nitrous gas, producing more or less acid according to the quantity of the oxygen gas. The solution of litmus, therefore, must acquire a red colour, and that, *cæteris paribus*, more or less, and in a shorter or longer time, according to the quantity of oxygen present in the water. When you intend to examine different kinds of water at the same time, after it has stood two days introduce into it carefully, in small quantities, by means of a syringe, an alkaline solution; and from the quantity of the solution necessary to restore the blue colour to the solution of litmus, which has been turned red, you can estimate very accurately the *relative* quantity of oxygen gas, and consequently that of the common air mixed with the water.

A proof of this kind was necessary in order to determine with certainty whether water could be entirely freed from oxygen gas, and consequently from common air; for azotic gas is never present in water alone.

Distilled water, however carefully prepared, and immediately received as it comes off in flasks closely stopped, cannot bear the proof by nitrous gas. Water freed from air by means of the air-pump again imbibes it as soon as the air has free access to it: even if the water be boiled in the flask, when exposed for a few moments to the atmosphere it becomes, in some measure, reddish by this proof.

After a great many fruitless attempts, I was never able to free water entirely from air but by the following process:

I boil pure water for at least two hours in a large basin; colour it with litmus; warm a large stoppered flask in it, and, holding it by means of a pair of forceps, fill it entirely with the boiling water. I let the flask remain for at least half an hour with its aperture some inches below the surface of the boiling water, frequently striking its sides with the forceps to expel any air which may still remain in it. I then invert the flask, place it in an earthen vessel, and immediately introduce nitrous gas: I then intercept all communication with the external air by mercury, of which there must be a sufficient quantity, on account of the diminution of the volume of the water, and particularly of the nitrous gas, by cooling; in consequence of which the mercury rises into the flask to a certain height. In this manner, I had the satisfaction to see that the liquor retains its blue colour for weeks; a certain proof that water can be entirely freed from air.

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I was now enabled to proceed a step further, and to determine whether after this process any of the *bases* of air, and, in particular, free oxygen, remained in the water, which during freezing might be extricated in the state of elastic air.

On one of the coldest evenings of the month of December, 1801, I filled some flasks, in the manner already mentioned, with blue-coloured water freed from its air and with nitrous gas, and others with water without any addition; all of which I placed in the window. Next morning no change was to be observed, though the frost had been very strong: the water in all the flasks was in a state of perfect fluidity. This phenomenon, I acknowledge, is very common: but I mention it because some consider the impeded disengagement of air as the cause of it; an idea which I was not ignorant of, and which agrees with the result of this experiment.

This circumstance rendered the experiment more agreeable; and being certain that every thing would freeze on the least agitation, I was enabled to observe better the smallest change which would take place during the process of freezing.

What I expected took place; none of those present during the freezing could observe the least change of colour. We every where saw bubbles appear, and disperse themselves widely through the lump of ice; but they were merely nitrous gas. At the end of two days we did not perceive the least trace of redness in the ice, till the flask at length bursting, and the air thus having free access, the whole mass instantly became red. The flasks which contained no nitrous gas, or tincture of litmus, exhibited during the freezing of the water no air-bubbles; but when the flasks burst in consequence of the frost increasing, the mass of ice was immediately interspersed here and there with air-bubbles.

But though these experiments fully proved my idea in regard to the origin of air-bubbles in ice, I was very desirous to observe the phenomena of freezing *in vacuo*.

My former pupil and worthy friend Van In En Kniphuysen, lord of Nienoord, who has added so much lustre to his noble birth by an assiduous cultivation of the sciences, was so kind as to assist me in these experiments, and to allow me the use of his valuable apparatus.

The experiment of Lichtenberg falls to the ground. We have seen curious phenomena. I shall give you an account of them, which I find in my memorandums, dated January 16, 1802. This will be more than sufficient to induce all the amateurs of the study of nature to repeat my experiments.

We had already observed several times, with great satisfaction,

tion, the spongy appearance which water acquires by freezing *in vacuo*, in consequence of its greater expansion, which every time that the glass burst was unaccompanied with the appearance of air-bubbles, but immediately on the introduction of air they spread themselves through the whole mass. Hence there was no doubt that this increased dilatation was entirely the consequence of the aëriform water, the expansion of which was not impeded by the pressure of the atmosphere. We repeated this experiment for the last time on the same day, when the frost was perceptibly less intense. A beer-glass capable of containing eight ounces was two-thirds filled with distilled water as much freed from air as possible: it was immediately placed under the receiver of an air-pump, and the air was exhausted as speedily as possible. At the end of some hours we saw a phænomenon which appeared so strange and singular, that I was at first incapable of saying what it might be. The freezing water had raised itself up in a straight column nearly three inches above the glass, being in thickness in the middle above a line, and at the bottom two lines. The water was equally frozen at the surface and throughout the whole mass: the cellular or spongy appearance existed only under the column, and extended to the middle of the base. No air-bubbles were any where to be seen. It is probable that the slowness of the freezing may have contributed to produce this singular phænomenon.

We conjectured, and with great probability, that this column was hollow, and had served for discharging the aëriform water. As soon as the air was admitted under the receiver we saw a movement in the pipe like that of air which penetrates into a wet glass tube, and in the mean time a great many air-bubbles appeared in the cavities at the bottom of the column. By introducing a stiff bristle into the aperture, and employing a magnifying glass, we could easily trace the hollow part of the tube to the cavity where the air-bubbles were.

The air, therefore, on this occasion had no share in the expansion of the ice; and as our water before freezing never contains so much air, it appears that during the freezing it could contribute little or nothing towards the expansion.

Expansion is a phænomenon common during all crystallization, and is a consequence of the peculiar regular form which the moleculeæ of bodies adhering mutually to each other assume, and by which means larger vacuities are formed. Expansion is increased, and particularly during the crystallization of water, by the aëriform state to which a part of it is brought by the disengaged caloric.

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While a part of this caloric, disengaged during the process of freezing, keeps the water beneath the crust of ice in a state of warmth and fluidity, another part, in consequence of the æriform expansion of the water, forms the cavities in the ice by which the so necessary communication between the external air and the fluid water is maintained, in order to support the life of those beings which reside in it.

And what would be the case with vegetation if ice were a body impermeable to air? While the warmth of the earth is maintained as far as possible under its hard surface by the power of congelation, the air continually penetrates through it; by which means the principles of germination are preserved in the seeds, and prepared for development.

My experiments have also shown that ice water produced by a slow thaw contains more air than water which has not been frozen during the same time.

But snow water in particular contains much more air than common rain water; and this air contains more oxygen than the air obtained from rain water.

The opinion of Hassenfratz, which may be seen in the fourth part of the *Journal Polytechnique* \*, is fully confirmed, whatever Dr. Carradori may assert to the contrary †. Snow water may, with great propriety, be called *oxygenated water*. This I can incontrovertibly prove, not only in the usual manner by sulphate of iron, but in particular by means of nitrous gas; and have shown it more than twenty times in my lectures, as a thing of importance not only to philosophy but also to pharmacy. What I assert may be easily proved in the following manner:

Fill two flasks, one with snow and the other with rain water, and place them, inverted, in boiling water: the air will then evidently be seen to collect itself at the bottom (now the top). Yesterday my amanuensis, H. Uilken, exhibited to us this important phænomenon. A flask filled with rain water, which at that time consisted chiefly of melted snow, did not present one-half of the air-bubbles exhibited by snow water collected in the garden.

Besides this larger quantity of air, and particularly oxygen gas, snow water contains also much fewer extractive particles than rain water; and from these two circumstances we can explain the properties by which this water distinguishes itself from rain water, as a medicine, as drink, and in many æconomical uses.

It is needless to inquire, why this water is sometimes hurt-

\* See Phil. Mag. vol. iii. p. 233.

† Ibid. vol. iv. p. 217.

ful in cases of inflammation of the eyes? why it occasions colic, griping pains, and other affections, when drunk cold? But, without enlarging further on this subject, I shall here mention a remarkable effect of the wise dispensations of Nature. As snow water contains oxygen united with little caloric, it thereby possesses a stronger tendency to communicate its oxygen to bodies susceptible of oxygenation. No substance in nature deprives water of its oxygen with more avidity than fertile earth. Snow water mixed with vegetable mould, and exposed to the solar light, improves the mould in a short time. Almost as soon as a lively fish placed in a glass of water containing oxygen makes the superfluous oxygen disappear, and fills the water with carbonic acid gas in its room, is the oxygen taken up by the earth assisted by the influence of the light. Pure earths do not exhaust snow water of its oxygen; nor do they attract the oxygen of the atmosphere, as asserted by Von Humboldt.

I have long been convinced of the contrary, from various experiments. It appears, in particular, that carbon is the principle whose strong affinity for oxygen produces so many important phænomena, and which nature continually employs in the composition and decomposition of organic bodies: it is carbon also which in this case deprives water of its oxygen, and consequently frees it from what renders it prejudicial to health, and unfit for the purposes of life.

That which is prejudicial to us is improved by the ground, and at the same time gives power and activity to the mould.

When the snow covers the earth, it tends to keep it warm: the snow, as a body which cannot conduct caloric in a very small degree, prevents, by its interposition, the cold air from taking the caloric from the earth. But this does not appear to be the only cause of its fertilizing power. The snow melting and penetrating into the softened earth communicates to it oxygen, promotes by these means the germination of seeds: the young plant grows with more vigour, because the carbon of the fertile earth combining with the oxygen is converted into carbonic acid, and thereby acquires more solubility; while the water, by its stimulating property, contributes to excite that activity which had been rendered dormant in the roots by the cold.

This fertilizing power of snow, which was before ascribed to nitrous particles, but the presence of which was never proved, seems thus, according to the idea of Ingenhousz, Hassenfratz, and other naturalists, to be explained in a more satisfactory manner.

I shall now conclude this short essay by expressing a hope  
that

that I have here given philosophers a further inducement to admire with me the great and noble operations of the divine providence, which so evidently appear in the different changes effected in air and water, and which are so necessary for supporting and maintaining the creation.

Groningen,  
February 26, 1803.

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XLV. *Observations on the Lotus of Egypt.* By ALIRE  
RAFFENAU-DELILE, *Member of the Institute of Egypt*\*.

SINCE my return from Egypt I presented to the Museum of Natural History different kinds of seeds and several bulbs of the *nymphæa cærulea*, of which an accurate description was given in Egypt by C. Savigny, my colleague. These bulbs began to germinate towards the end of Messidor, when kept immersed in water. I collected them in the island of Rosetta in the month of Ventose, year 8; and though I had kept them two years without planting them, they had not become desiccated: one of the plants of the *nymphæa* has already produced several flowers somewhat smaller, less odorous, and of a paler blue colour, than those in Egypt.

Though I made a tour with the Commission of Arts into Upper Egypt during the time of the inundation, which is that also of the increase of the two kinds of *nymphæa* of the Nile, we met with none of these plants in that country.

The waters of the Nile rise to a considerable height in the Said, and much less in Lower Egypt. The *nymphæa*, therefore, easily reaches the surface of the water, during the inundation, in the lakes and the canals of the Delta, where they are not hurt by the current of the river. It is, perhaps, for a contrary reason that the *nymphæa* does not grow in the Said.

We observed the *nymphæa cærulea* painted and engraven on the Egyptian monuments much oftener than the two kinds of lotus mentioned by the oldest historians. This *nymphæa* has a great resemblance to these kinds of lotus, and appears to be a third species of it.

Most authors who have undertaken to make known the lotus in regard to its antiquity have fallen into many errors; but Mahudel, of the Academy of Inscriptions and Belles

\* From *Annales du Museum National d'Histoire Naturelle*, No. 5.

Lettres \*, and lately professor Sprengel, of Halle †, have given very accurate information respecting these plants.

The expedition to Egypt afforded me an opportunity of making the following observations on the same subject: they are naturally connected with those already made in Egypt by C. Savigny on the *nymphæa cærulea*, and complete the history of that plant.

Herodotus ‡ relates that the Egyptians fed on the lotus which grew in the waters of the Nile. He calls this lotus a lily, and says that the seeds, similar to those of the poppy, were employed for making bread. He adds, that they ate also the roots of the lotus, which were round, of the size of an apple, and of a sweet and agreeable flavour. Herodotus then speaks of another kind of lily resembling a rose, which grew also in the Nile, the fruit of which had the form of a wasp's nest, and which contained several seeds good to eat, and of the size of an olive.

Theophrastus § gives a very accurate description of the same plants:—"The lotus," says he, "grows in the fields when they are inundated. Its flowers are white, and have petals like those of the lily. They spring up in great numbers close to each other. They shut their leaves at sun-set, and conceal their fruit. These flowers open again when the sun reappears, and rise above the water; and this alternation is repeated till the fruit is completely formed, and the flowers have dropped off. The fruit is equal to that of a large poppy, and contains a great number of seeds similar to grains of millet. The Egyptians deposit the fruit in heaps, and suffer the rind to putrefy; they then separate the seeds by washing them in the Nile, dry them, and knead them into bread. The root of the lotus called *corfion* is round, and of the size of a quince. Its rind is black, and like that of the chestnut. This root is white in the inside, and is eaten either raw or boiled."

The lily of the Nile, like the roses mentioned by Herodotus, is called by Athenæus || Egyptian bean, and the rose lotus. Theophrastus gives it the name of *bean*.

"This bean grows in the marshes and ponds. Its stem is four cubits in length, and as thick as the finger. It resembles a reed without knots. The fruit it bears is shaped like a wasp's nest, and contains about thirty beans a little prominent, each deposited in a different cell. The flower is twice as large as that of the poppy, and entirely a rose. The leaves are sup-

\* Histoire de l'Academie des Inscriptions, vol. ii. p. 285.

† An iquitatum botanicarum Specimen primum, p. 47.

‡ In Euterpe, cap. xcii.

§ Theophrasti Hist. Plant. lib. iv. cap. 10.

|| Athenæi Deipnosoph. lib. iii. p. 72; lib. xv. p. 677.

ported by pedicles similar to those of fruits: they are large, and have a resemblance to a Thessalian cap. When one of the beans is bruised there is seen in the inside of it a small body folded together, from which the leaf arises. The root is thicker than that of a strong reed, and has partitions like the stem. It serves as nourishment to those who reside near the marshes. This plant grows spontaneously, and in abundance. It is sown also in the mud, by forming for it a bed of straw, in order that it may not rot."

The lotus of Herodotus and Theophrastus grows also in Egypt. It is the *nymphæa lotus* of Linnæus\*, the characters of which, compared by C. Savigny to those of the *nymphæa cærulea*, agree with the descriptions given by the antients.

In regard to the rose lily or Egyptian bean, which is frequently carved out among the ornaments and symbolical representations of the Egyptian temples, it no longer grows in that country; and would have remained unknown to naturalists had it not been discovered in India. This plant has been called by Linnæus† the *nymphæa nelumbo*. Its fruit, which the Greeks compare to a wasp's nest, has a perfect resemblance to it. They called it *ciborion*‡ on account, no doubt, of its resemblance to a cup. This fruit is shaped like the spout of a watering-pot, and is flatted at the upper part, which contains from fifteen to thirty small fossæ, containing a like number of seeds of the size of a nut, and somewhat prominent. The plumula§, which is the rudiment of the leaves, is, indeed, rolled up in the middle of the seed, as Theophrastus says. The flowers are roses, and very large. The leaves are orbicular, thuriform, and about two feet in diameter||.

Belon has confounded the Egyptian bean with the colocasia¶ (arum colocasia Linn.), which is cultivated in Egypt: as the Greeks often gave the name of *colocasia* to the root of the Egyptian bean\*\*, it was a difficult matter to avoid confounding these two plants.

Sprengel remarks, that the appellation of *colocasia* was given also to the *nymphæa lotus*††.

But we receive as much information respecting these plants

\* Species Plantarum, p. 729.

† Ibid. p. 730.

‡ Athenæi Deipnosoph. lib. iii. p. 72; & lib. xi. p. 477. Diodorus Siculus, lib. i. Dioscorides, lib. ii. cap. 97.

§ Gærtner De Fructibus, &c. tom. i. p. 74. tab. 19.

|| Hortus Malabar. tom. xi. p. 61.

¶ Belon, Observations, lib. ii. chap. 28.

\*\* Athenæi Deipnosoph. lib. iii. p. 72 & 73.

†† Antiquitat. botanic. Specimen prim. c. 89.

from antient sculptures as from the account of historians. The rose lotus, or Egyptian bean, is very correctly represented in the mosaic of Palestrine, an explanation of which has been given by Barthelemy in the *Memoirs of the Academy of Inscriptions and Belles-Lettres* \*. The fruit, the flowers, and the leaves of that plant are exceedingly like. They float on the surface of the water in a lake which carries several barks during the time of a festival. This painting brings to remembrance a passage of Strabo †, who says, that people made excursions of pleasure in boats on the lakes covered with beans, and that they sheltered themselves from the sun with the leaves of that plant. On the Egyptian monuments Harpocrates is represented above the flower of the fruit of the rose lotus. This plant, so well known in antient Egypt, is at present celebrated in the religion of the Bramins, and is often placed among the attributes of the Indian deities ‡.

It is not possible to say to what kind of lotus the flowers seen represented on the heads of the Egyptian kings or deities in several medals belonged, because the species of lotus differ chiefly in regard to the colour of their flowers, and the form of their fruits or leaves; but on the walls of the temples of Egypt, and on the cases containing the mummies, they may be easily distinguished when the painting is in good preservation. The Egyptians often represented the leaves of the white lotus (*nymphæa lotus*) of the same size as the flowers, though naturally the leaves are much larger; but they have omitted to mark the indentations of these leaves, which indeed are wanting when the plant is very young. I have, however, seen in the table of Latopolis this lotus represented with the leaves indented.

But it would be in vain to seek for scrupulous exactness in allegorical sculptures. Thus on the base of the statue of the Nile, placed in the gardens of the Tuileries, the fruit of the lotus is very exactly represented, but the leaves which accompany it are not those of that plant.

The fruit of the white lotus, which has the same form as

\* *Histoire de l'Acad. des Inscript.* 1790. The *Pitture antiche di Pietro S. Bartholi*, which represent the mosaic with its colours, may be seen in the library of the Pantheon.

† *Strabo*, lib. xvii. The Greek text has been badly interpreted by translators, who supposed that the boats rowed along under the shade of the leaves, which rose to a great height above the water. These leaves float on the surface of the water, and are very broad. It appears that the boats were covered with them in order to form a shade, as they are covered in Egypt at present with the leaves of the date-tree and with reeds.

‡ *Systema Brahmanicum Fr. Paullinij*, à Bartholom. tab. 9 & 10.

that of the poppy, may be distinguished on the Egyptian monuments. In my opinion, this resemblance has made the fruit of the lotus, delineated on several Egyptian medals, to be confounded with that of the poppy. The fruits represented on these medals are the same as those sculptured on the Egyptian monuments anterior to those of the Greeks. It does not appear from any historical testimony that the Egyptians ever made much use of the poppy; and they rather placed the fruits of the lotus among the attributes of Isis, with ears of corn, as a sign of abundance and fertility, since they were long accustomed to make a sort of bread with the seeds of that plant. The Egyptian lotus was very little known to the Greeks and the Romans, who compared it to the most common plants. Herodotus calls the lotus a *lily*; Theophrastus compared it to the *poppy*, and Pliny calls its flowers *poppies* \*. Another cause which may have occasioned the lotus to be confounded with the poppy, is the resemblance which exists between the attributes of Isis and those of Ceres, to whom the poppy was consecrated †.

The *nymphæa lotus* and the *nymphæa nelumbo* are the two species of lotus described by Herodotus and Theophrastus. Both grew naturally in Egypt, and were cultivated in that country. It was the fruit of the cultivated *nymphæa lotus* to which Pliny gives the name of *lotometra* ‡.

A passage in Athenæus proves that the *nymphæa cærulea* is a third species of lotus. This author relates that Antinoian § crowns were made at Alexandria with the rose lotus, and that the blue were interwoven with these crowns. The flowers of these different kinds of lotus are very odoriferous, have great splendour and freshness, and must have been selected for making crowns. Heliodorus relates, that couriers who announced a victory in Meroë were crowned with lotus ||. When Plutarch speaks of a crown of melilot which fell from the head of Osiris ¶, and when he classes that plant among those which grow in the Nile, he alludes to a crown of lotus. Athenæus relates, that the lotus was also called melilot \*\*, and that it was formed into melilotine garlands. The same historian tells us also why the rose lotus was called the Antinoian. A poet presented the emperor Adrian, during

\* Plin. Hist. Nat. lib. xiii. cap. 17.

† Virgil. Georg. i. ver. 212. Ovid. Fast. lib. iv. Theoc. Idyll. vii. ver. 153. Callimach. Hymn. Cer. ver. 45.

‡ Plin. Hist. Nat. lib. xxii. cap. 21.

§ Athenæi Deipnosoph. lib. xv. p. 677.

|| Heliodor. Ethiop. lib. x. cap. 28.

¶ Treatise on Isis and Osiris.

\*\* Athenæi Deipnosoph. lib. iii. p. 73.

his residence at Alexandria, with a rose lotus as a rarity, and said that this lotus, which had grown up in a country moistened by the blood of so terrible a lion, ought to be called the Antinoian. The lion of which the poet here speaks had ravaged a part of Libya, and had at length been destroyed by the emperor Adrian during a hunting excursion.

Independently of the plausible conjectures of several writers respecting the origin of the religious employment which the Egyptians made of the lotus, these plants, on account of their simple and natural properties, must have been very much celebrated in antient Egypt. This country being indebted to the Nile for its prosperity, its inhabitants considered as the signs of a great benefit the plants which grew in the waters of that river. The flowers of the lotus rise to the surface of the water when the Nile begins to increase, and announce the inundation, which is about to bring abundance along with it. Besides the names of *bachenin* and *naufar*, which the Egyptians give to the nymphæa, they call them also *araïs el Nil*, that is to say, which grow up in the Nile. These names certainly refer to the fertility about to be renewed by the presence of the waters.

The Egyptians collected the roots of the lotus when the waters of the Nile retired. At present they are seldom collected; but they multiply very much in the rice-fields, so that the peasants are obliged to pull them up after the rice harvest. They then sometimes eat these roots, which they call *biaro*. I saw some of them sold in the market of Damietta in the month of Frimaire, year 7: I tasted them, and found that their taste was not disagreeable. These roots are round, or somewhat oblong, and smaller than a common egg. The rind is black, and coriaceous: they exhibit tubercles, traced out by the base of the petioles or shoots. These roots in the inside are white and farinaceous; in the centre they are yellowish. After the inundation they remain sunk in the earth which has become dry; and the following year, when covered by water, they send forth leaves and small roots merely from the summit, which has a cottony appearance. The radiculæ penetrate laterally into the mud, where they produce tubercles which become similar to the former roots, and which multiply the plant. The Egyptians to collect the seeds washed them, after leaving the rind of the fruit to putrefy. This method is the only one that could be employed, otherwise these seeds would become mixed and desiccated with the parenchyme of the fruit. These seeds are very small, rose-coloured or gray on the outside, and farinaceous within. The antients compared them to grains of millet. I have  
heard

heard the peasants call them *dochn el bachenin*, that is to say, millet of bachenin: but they told me that they were of very little use. The roots and seeds of the *nymphæa lotus* and those of the *nymphæa cærulea* are similar. It is natural, then, to believe that the Egyptians not only made garlands of the blue lotus, but that they used it for food like the *nymphæa lotus*. This is proved by the Egyptian sculptures, since the blue nymphæa is often represented among the offerings of fruits in the grottos of Said, the paintings of which exhibit scenes of domestic life.

Of the two kinds of nymphæa, the Egyptians at present prefer that with blue flowers, which is often represented in the temples. Ebn il Bitar, an Arabian physician, who has written a treatise on plants, quoted by Prosper Alpinus \*, distinguishes two kinds of *bachenin* or *nymphæa*, the best of which is called that of the Arabs. I observed that the peasants of the Delta gave the name of bachenin of the Arabs to the *nymphæa cærulea*, and that they set less value on the *nymphæa lotus*.

As the antients have made very little mention of the blue lotus, one might believe that it was brought to Egypt from the East Indies along with the rice, since it grows abundantly in the rice-fields of the Delta; but the paintings in the temples evidently prove that this plant is as antient in Egypt as the *nymphæa lotus*.

It is certain that the *nymphæa cærulea* exists in India. In the *Hortus Malabaricus* it is called *citambel* †. Rumphius considers it as a variety with blue flowers of the *nymphæa lotus* ‡. This nymphæa grows also at the Cape of Good Hope, and seems to be sufficiently distinguished by the following phrase of Breyn: *nymphæa flore cæruleo odoratissimo Capitis Bonæ Spei* §. A figure of it has been given by Andrews ||.

\* Prosperi Alpini Rerum Egypt. lib. iii. cap. 10.

† Tom. ii. p. 53. tab. 27.

‡ Herbar. Amboin. tom. vi. p. 72.

§ Prodrom. ii. 26.

|| Botanists Repository, 197.

XLVI. *Memoir on some Peculiarities in the Anatomy and Physiology of the Shark, particularly respecting the Production of its Young.* By Dr. MITCHILL, of New York\*.

ALTHOUGH the generation and multiplication of animals have so long exercised the attention of philosophers, the whole subject remains involved in intricacy and indistinctness. The maxim laid down by Harvey, and adopted by Linnæus, of *omne animal ex ovo*, that every animal proceeds from an egg, has, perhaps, been too generally received, since numerous facts, related by Mr. Bonnet, in his work entitled *Considérations sur les Corps Organisés*, evince the propagation of animals, in a great variety of cases, from germs. Notwithstanding the multiplicative powers of animals have been thus traced to eggs and germs, yet a popular distinction still prevails to a considerable extent, of classing animals that propagate their species by means of genital organs, into oviparous and viviparous.

The great discovery of Haller, that the membrane covering the yolk of an egg was really a continuation of the membrane covering the intestines of the chick, had not only given countenance to the idea of the pre-existence of the embryo, but has shown, that animals, whether of the oviparous or viviparous kinds, really propagated their species in pretty much the same way.

Amidst the different modes in which the embryo and its membranes are organized in different animals, there seems to be one case which has not been hitherto described with the accuracy and minuteness which its singularity deserves: the genus of the squalus, which includes all the animals of the shark tribe, has some peculiarities which make these animals approach both to the oviparous and viviparous classes, without, however, belonging strictly to either.

It had been known a long time, that the young of the shark had something in their structure considerably different from any other creatures, and figures of them have been given by Edwards in his Natural History, and probably copied from thence into the Encyclopædia; but there has not been, as yet, any dissection of these animals in this period of their existence, nor any explanation of their physiology that I know of. It is the object of this short memoir to explain the structure and functions of the fœtus of a species of shark found frequently along the coast of New-York, in the waters of the Atlantic, during the summer months. About two years

\* Communicated by the Author.

ago, as I was engaged in a fishing party in one of the bays on the south side of Long Island, a shark, between four and five feet in length, was taken in the seine, and secured in our boat, without receiving any material injury. Upon examination, this animal was found to be a female, whose uterus contained eleven young ones, of the size and figure represented in the plate.

Besides these young ones that had advanced thus far in their growth, there were contained a large number of ova within the body of this fish, in different degrees of evolution and size, some of them resembling the full grown eggs of the tortoise, and others similar to the smaller rudiments of eggs found in the ovaria of laying hens. On opening the uterus with a knife, the young fishes, as represented in the figure, were found each connected with an egg, dependent from that part of the belly which may be considered as the umbilicus, and appearing in the form of a very large hernia. This hernia, on examination, proved to be a true ovum, filled with yolky substance, evidently intended for its nourishment: and what was very remarkable, the young animal, though grown to a considerable size, and connected in this manner with its egg, had no connection whatever by means of an umbilical cord, a placenta, or by vessels of any kind, to the uterus of its dam; but it was so completely organized as to derive no sustenance to its body, nor to receive any renovation of its blood from its parent.

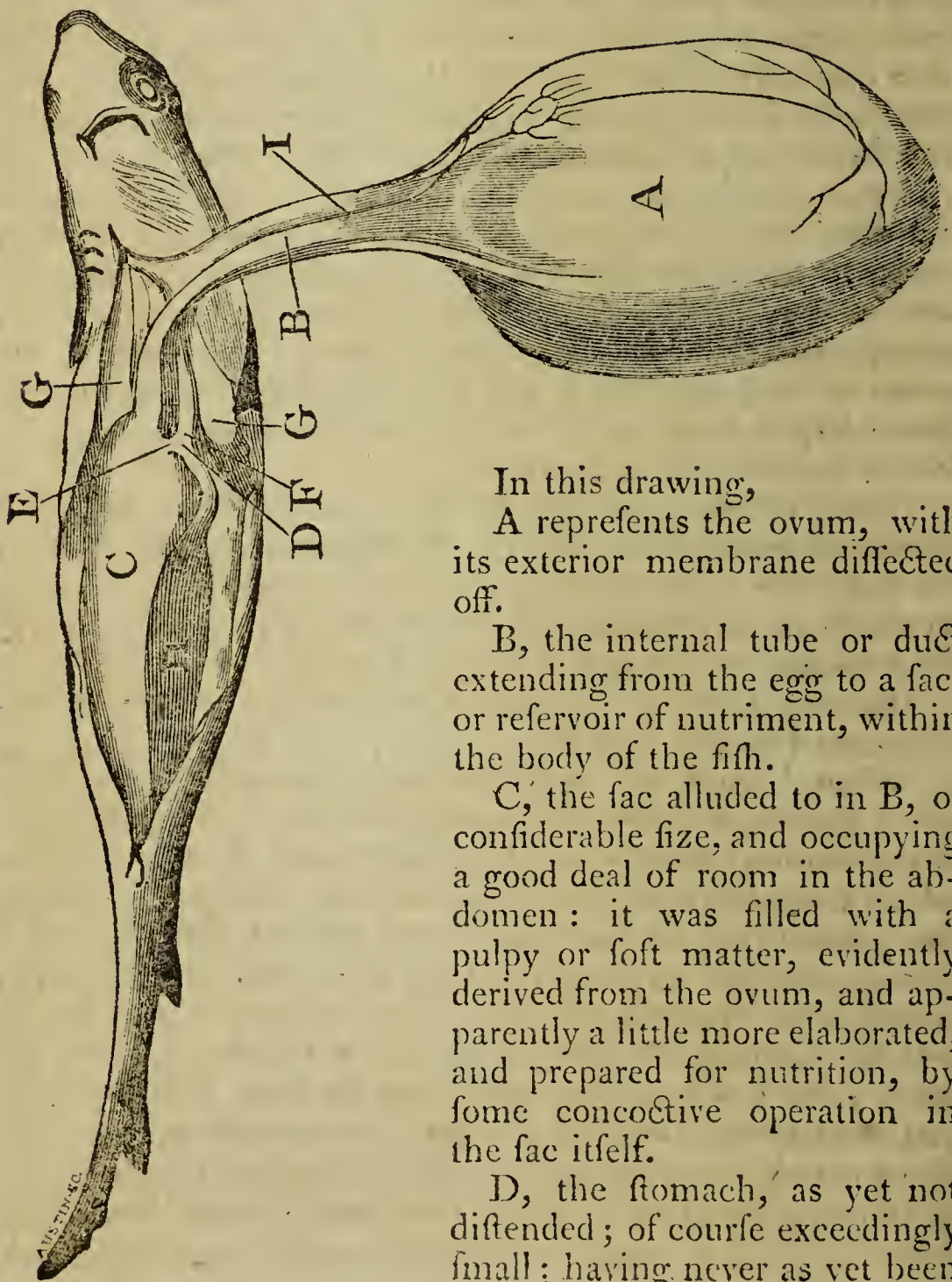
The membrane covering the egg contained most beautiful ramifications of blood-vessels. The arteries descending from the little fish could be seen sending off their branches over its whole exterior surface; and terminating in veins, to unite their trunks, and carry back their contents.

The singularity of all these appearances was considerably heightened by the capability of the little sharks, when cut out of the uterus, to live for a considerable time in the open air. The larger part of the brood had been left on the grass of the shore where the dam had been dissected; but the three which I reserved for examination lived, and exhibited, during the greater part of the time, brisk motion for almost two hours, although exposed to the temperature of a common atmosphere. During this time, while they lay before me on a plate, nothing of the kind appeared more beautiful or distinct than the branches of blood-vessels shooting through and running over the transparent membrane of the egg, the blood evidently appearing to acquire a brighter scarlet colour, whilst the fish was thus exposed to the air, than it had possessed during the immersion of the young animal in the fluid of the uterus. It seemed to have acquired more rapidly, and

to

to a greater quantity than before exclusion from its maternal membrane, the oxygen of the air to which it was exposed; the union of which with the blood evidently brightened its colour, and imparted to it at the same time so much of a stimulant quality as to have shortened the duration of its life by excessive excitement.

The internal structure of the foetus of this shark may be seen in the plate. The dissection was made by my colleague Mr. Post, professor of anatomy in Columbia College; and the drawing was executed, immediately after, by Dr. Alexander Anderson, of New York, in the presence of the late Dr. Elihu H. Smith, Mr. William Dunlap, and Mr. Thorne.



In this drawing,  
A represents the ovum, with its exterior membrane dissected off.

B, the internal tube or duct extending from the egg to a sac, or reservoir of nutriment, within the body of the fish.

C, the sac alluded to in B, of considerable size, and occupying a good deal of room in the abdomen: it was filled with a pulpy or soft matter, evidently derived from the ovum, and apparently a little more elaborated, and prepared for nutrition, by some concoctive operation in the sac itself.

D, the stomach, as yet not distended; of course exceedingly small; having never as yet been filled

filled with food derived from the mouth. In the plate will be seen a small passage or opening,

E, through which the alimentary matter in the sac, C, passes into the stomach, to be absorbed afterward by the lacteals of the intestines.

F, the œsophagus, of nearly the same size with the stomach.

GG, the two lobes of the liver.

H, the cloaca, or gut stretching towards the anus, and filled with the refuse matter of the alimentary mass.

I, the external membrane connecting the egg with the fish, cut through and turned back.

A species of shark, called *catulus major vulgaris*, is delineated by Edwards, but appears to be different from this. He has taken two views of the parent animal, and two more of the young. From the figures there given, it would seem that the same law of generation obtained as in this species. All the species probably breed in the same way.

There is a great variety in the multiplicative process of living beings. The female *rana pipa*, or Surinam frog, nourishes its young in certain cells or cavities in her back, and not in the womb. The *opossum* of this country supports her young appendant to the teats within the paunch or sac, called a false belly. The *kangaroo* of New Holland has somewhat of a similar structure and œconomy. More extensive and penetrating inquiries show the exceptions to the common mode of generation to be almost indescribably curious and diversified. We see no end to the variety of ways in which the perpetuation of the species is carried on. Even Spallanzani (5 *Viaggi alle due Sicilie*, &c. p. 46), though he went to the lake of Orbitello on purpose to examine the anatomy of the large eels which live there, could discover in them no appearance of sex.

When I published my Inaugural Dissertation in 1786, *Circa novi Genituram Animalis*, I was decidedly in favour of the hypothesis of pre-existent germs, and of the production of animals by their gradual evolution. All my numerous experiments on generation appeared at that time to lead to such a conclusion, though I have since had a good deal of reason to doubt the soundness of the inference; and the experiment now to be related, though it throws light on some part of the process, leaves the main question almost as unsettled as ever. In 1789 I ordered a large sow to be killed, immediately after having had intercourse with a male. On examining the genital organs, the blood-vessels of the vagina, uterus, fallopian tubes, and simbrizæ, were more than usually distended,

distended, and the fimbriæ in particular were in a condition of high redness and inflammation. Their fibres were lengthened, and reached so far as, on one side, to embrace the whole ovarium, and contain it within their inclosure. But the most remarkable appearances were in the ovarium itself. The sow had borne pigs before. Some of the ova were of course exhausted of their powers, and exhibited the appearance common in such cases. The whole ovarium was tinged with blood, and appeared to have been under the operation of a powerful stimulus. The entire substance seemed to have been enlarged. The ova partook of this enlargement, and all of them had evidently shared in the excitement and suffusion. Some of them were but little swelled: some were so tumid as to be on the point of bursting: the membrane of others was ruptured, and the contents partly protruded; and, in several, the substance discharged from the broken ovum was fairly within the grasp of the fimbriæ. I imagined now I had before me proof sufficient to determine the derivation of the foetus from the mother. I accordingly examined the portions of substance discharged from one ovum, and extracted from others, with all possible care. But instead of finding an embryo, or any thing like the rudiment of a young animal, the little masses I had obtained resembled coagulated blood more than any thing to which I can compare them, and appeared to have no more of organization or figure than is frequently seen in grumes or clots of that fluid.

**XLVII.** *On the Management and Improvement, by Tillage, of old Grass Lands on a direct Clay, such as is found in the Wilds of Surrey and Sussex\*.*

**T**HE substratum to this soil being impervious to water, the surface must consequently be continually saturated with moisture. Many landlords on such soils restrict their tenants, by covenants, not to till or plough up the old grass lands, under an apprehension of the soil being exhausted by tillage and cropping. In this unproductive state the land lets for from five shillings to seven shillings and sixpence an acre; and, being always full of stagnant water, the produce consists of poisonous weeds, water quitch, and a little four grass. Two acres will keep one head of lean cattle alive for

\* From Mr. Close's Paper in the Communications to the Board of Agriculture, vol. iii. part 1. We gave a former part of this paper in our last Number, p. 167.

twenty spring and summer weeks. Some labour, and a considerable expense, will be requisite to bring this stubborn, sterile soil to a state of fertility.

I would first recommend the surface to be pared and burned, and the ashes to be secured dry upon the headlands of the fields, by a covering of earth, sods, or thatch. This operation must be finished in the summer. As soon as the autumnal rains have moistened the soil so as to enable a plough to turn a furrow seven or eight inches deep, this operation should be performed, forming each ridge with only two furrows, three feet from the centre of one ridge to the centre of the other. Then let the scarifier, with three long tines, or teeth, pass at the bottom of every furrow to loosen the soil five or six inches deeper. Water furrows should be cut so as to lay the land dry all the winter. Should the winter prove favourable, the tops of these ridges may be harrowed down with the fixed harrow, and the scarifier passed again through the bottom of the furrows, and the ridge formed over this deeply pulverized soil: if not, they must remain until perfectly dry in the spring. In tilling soils of all denominations in which the clay predominates, this general rule should be observed; no injury will be sustained from ploughing them when wet, previous to the winter frosts: but all the horses on a farm had better remain idle than touch a furrow in the spring, until the soil be perfectly dry. When in this latter state, harrow down the tops of the ridges; scarify, with Mr. Cooke's scarifier, the bottoms, and once more reverse the lands, forming the ridges where the furrows were. Thus a depth of fifteen inches of finely pulverized soil will be obtained by two half ploughings, two scarifyings, and two harrowings; at least the soil will be so much opened to that depth as to admit the rain water to pass freely. Every shower will not make a paste on the surface, neither will forty-eight hours sun form it into bricks. In short, by stirring this land deep you avoid the injuries to which it was before exposed by excessive wet or drought, and the soil will become healthy and productive. Should it not be sufficiently pulverized for turnips, scarify, harrow, and roll, with a heavy roller only five feet long, until a garden tilth be obtained, leaving the land, when completely fine, on three-foot ridges. Previous to putting ashes on for drilling turnips, beat down three or four inches of the tops of the ridges with a sled, or beam of wood five feet long, annexed to the handles of Mr. Cooke's scarifier. On this bed of finely pulverized mould in the furrows spread the ashes; about eighty bushels on an acre. This work will be easily performed, as the horse will walk in the  
furrow,

furrow, and each wheel of the cart will occupy another. As the horses and the cart-wheels will make the soil in the bottom of the furrows immediately under the ashes compact, pass the scarificator through, as before directed, with only three long tines, or teeth, in the centre of the beam, then with one bout of the common plough reverse the ridges and cover the ashes. Let the land continue in this state until the season suit for sowing. Then with the sled, as before, level the tops of the ridges so as to obtain a surface on each ridge eighteen inches wide. Mr. Cooke's drill will sow two of these ridges at a time, two rows of turnips on each ridge, nine inches from row to row, with an interval of twenty-seven inches. The turnips, as soon as the rough leaf appears, should be harrowed across the ridges with the fixed harrow, then scarified between the rows; and a few days after these operations they should be hand-hoed, leaving the plants in rows, single, and one foot apart. The intervals should be kept highly pulverized by alternate ploughings, horse-hoeings, and scarifying; being careful, before the autumnal rains commence, to throw all the mould from the bottoms of the furrows up to the ridges with a double mould board plough. This will not only keep the land dry and healthy all the winter; but, as the horses will walk in one furrow, and each wheel of the cart will occupy another, the turnips may be carted to the bullock-sheds without poaching or injuring the land.

Knowing the difficulty in these clayey soils of obtaining a fine tilth for spring sowing, I should recommend the same ridges, after the turnips are drawn, and carted home, to be worked with the fixed harrow and roller, only so as to obtain a fine tilth five inches deep; and then to drill two rows of oats, nine inches from row to row, leaving a furrow or interval of twenty-seven inches. When the oats are about two inches above the surface, harrow across with the fixed harrow, scarify twice in a place between the rows of oats, and roll the whole. After these operations, should weeds appear, horse-hoe with the flat hoes. When the oats are harvested, throw your ridges up high, like trenching in a garden, without reversing them, keeping the bottoms of the furrows clean from mould, and laying the land perfectly dry. In the first frost cart about twenty loads per acre of good horse or bullock dung, not too rotten, into the furrows; spread it; and as soon as the frost is out of the land, harrow or sled down the tops of the ridges, reverse them, and cover the dung: the first dry weather in February or March, sled down the tops of these ridges, to obtain a surface of eighteen inches, and drill two

rows of beans on each ridge, nine inches from row to row, and twenty-seven inches interval. Scarify, horse-hoe, and plough between the intervals, so as to keep a fine tilth, and to destroy the weeds. When the beans are fit for harvesting pull them, and when carried off the land pulverize the tops of the same ridges by scarifying, rolling, and working with the fixed harrow; and, if any surface-weeds appear, skim the land with the scufflers; and by repeating these operations prepare the same ridges for wheat, and drill two rows on each ridge nine inches from row to row. In the spring harrow across, &c. as directed for the oat crop. Should the farmer wish to substitute cabbages for turnips, one row should be planted on each ridge, setting the plants two feet and a half from each other in rows, with three-feet intervals. In this succession of crops, viz. turnips or cabbages, oats or barley, beans, pease and clover, and wheat, even this naturally sterile soil will be in a constant state of improvement: and if the vegetable crops and straw be expended in the most advantageous method upon the farm, the land may be dressed with fourteen loads per acre twice in four years, viz. for the turnip or cabbage crop, and for the bean, pea, and clover crop. I shall not, therefore, limit the number of years for keeping it in tillage to any precise term, as it appears to me immaterial; but proceed to point out the best method of securing a good meadow whenever the occupier may wish it\*.

We will suppose the land cropped eight years in the rotation of crops before recommended, and at the expiration of that term, a wheat stubble. The first operation I should recommend would be to drain the land so as to take off the surface water, for nothing more can be effected by draining these lands, as they will hold water like a bucket; and were the drains cut ever so deep, and only six inches from each other, the water would not draw from one to the other. The method I shall propose is easy and efficacious, and the expense very trifling. I speak with confidence, as almost the whole parish of Hitcham, in Suffolk, where twenty years back it was thought impossible to grow barley, now produces fine and heavy crops of that grain, by the surface water being taken off expeditiously. Open furrows in the wheat stubble with the common plough, about five yards from each other, and as deep as the land was ever ploughed and scarified. Then with a narrow gripping-spade, twenty inches long,

\* Old grass lands converted into tillage, though they might be ploughed by a judicious system of cropping for any number of years, if naturally crude and unproductive, should not, I think, be sown with grass seeds in less than eight years.—*Note by Mr. Close.*

and

and only two inches and a half wide at the top, and one at the bottom, open a drain sixteen inches deep; fill this with the wheat haulm, ramming it down as hard as possible; the price two shillings and sixpence per score rods, to dig and fill up. It is, I presume, unnecessary to observe that these and all other drains should not be cut in a line with the natural fall of the land, but in an oblique direction. No bushes should be used, as, when these begin to decay, pieces of them will fall across these very narrow drains, and obstruct the course of the water. The straw or haulm, when it decays, sloughs off, and the water passing through the clay forms an arch, which will last many years. There are drains of this sort still open in Hitcham parish, which have been cut more than twenty years. Having thus freed the land from the noxious moisture arising from stagnant water, with one bout of the plough throw the land on to three-foot ridges, and before January reverse these. In the spring, when perfectly dry, harrow down the ridges with the fixed harrow, so as to make a complete fallow\*; then dress with lime or compost, and baulk the land, viz. give it half a ploughing, leaving it in narrow ridges, so as it will harrow down quite level and fine without any furrows appearing. In the beginning of August sow thirty-two bushels per acre of the sweepings of hay-chambers, where only good old meadow hay has been used without any broad clover and rye-grass, five pounds of Dutch clover, and five pounds of trefoil; harrow the seeds three or four times over, and you may expect two tons or two tons and a half of hay per acre the first year; which will pay all the expenses, and leave the land improved from five shillings or seven shillings and six-pence per acre, its original value, to fourteen shillings or sixteen shillings per acre.

And now, gentlemen, as you seem particularly desirous of obtaining information from actual experiment, permit me to assure you, that though I have not given you, neither is it in my power, an exact detailed account of an experiment I made on fifty acres of land in the wilds of Surrey, yet the great outlines of the plan, and the general result, correspond with my actual practice. I purchased nearly 600 acres of land in that county for three thousand six hundred pounds; the stock and crops were valued to me at about five hundred pounds, and the rent of the major part of the land estimated at five shillings per acre. After farming it some years,

\* The scarifiers would be dangerous instruments to use after the drains are cut, as they, by penetrating too deep, might pull down the sides of the drains, and bring the haulm to the surface.—*Note by Mr. G/ose.*

I let the whole at ten shillings per acre; the tenant paid me for the stock and crops two thousand and eighty pounds; and I afterwards sold the estate to him at nearly double the price I gave for it, having previously paid myself the greater part of the expenses for the improvements I had made. Fifty acres of the land will more particularly elucidate this subject. Part of it was broken up from old four grafs, not worth five shillings per acre, and part of it was old tillage land, which had been limed and cropped until it literally would produce no weeds except the sow-thistle. After treating these lands, as nearly as I can recollect, according to the system I have recommended, I fallowed and dressed them with a compost of lime and earth, and sowed the quantity of seeds I have mentioned, at or near the season recommended, and the produce was estimated at two tons and a half of hay per acre; and the fifty acres of land which were valued at five shillings per acre when I purchased them, were estimated at fifteen shillings per acre when I sold the estate. I did not surface-drain the land, as I was not then acquainted with the system, neither did I pare and burn, but dressed with a compost of lime and mould. The system I have recommended differs from my actual experiment in these two circumstances. But the surface-draining has proved effectual in the parish of Hitcham, and seems to carry conviction with it; and by paring and burning, as good a dressing as the lime may be procured, at a much less expense. Many may object to paring and burning and ploughing these lands into what is termed, by the farmers, the dead earth. I am of opinion these operations are pernicious, on really thin-skinned soils, when the immediate substratum is much inferior to the surface. But in the soil I have been treating of, the only difference between the surface and the substratum consists in the former having been exposed and pulverized, and the latter having remained impregnated with stagnant water, and never turned up so as to derive any benefit from the vicissitudes of the atmosphere. By paring and burning you destroy the surface-weeds, and obtain a cheap dressing; by ploughing the land deeper than ever it was ploughed before, exposing it to a winter's frost and summer's sun, and dressing it with ashes or lime, you obtain a depth of sweet mellow soil, not easily injured by wet or drought, and adapted to all the purposes of vegetation. To prove the utility of moving these clay soils fourteen or fifteen inches deep, let any one cover a pewter dish, or any substance like the substratum of these lands, impervious to water, with earth one inch deep, and expose it to a trifling shower, and the whole mass will

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become a quagmire ; let another dish be covered with the same soil fifteen inches thick, and it is evident it will require fifteen times the number of cubic inches of water to put this into the same boggy state. The former also by twenty-four hours dry weather will become as hard as a brick, when the other will only have a slight incrustation formed on the surface. Supposing this effect to be produced on your field, by passing the fixed harrow once across the land, it will be put into a state of fine pulverization. By drilling on three feet ridges, you keep the land in a dry healthy state ; and by sowing your spring crop on the same ridges, you obviate the only objections to growing green winter crops on wet lands, viz. cutting up the lands by carting off the crop, and the impracticability of afterwards preparing them for spring sowing. In carting, the horses walk in one furrow, and the wheels track in two other furrows ; the tops of the ridges are always dry ; and the fixed harrow passing twice in a place, will prepare such lands for sowing, before the farmers in the common mode of management, without a crop of green food, can put a hoof on their lands. Should surface weeds appear before sowing, Mr. Cooke's scufflers, or broad shares fixed in the scarifying-beam, should be passed through the land.

[To be continued.]

Erratum.—In the last Number, p. 168, last line, for Cole read Close.

XLVIII. *Some Account of* GEORGE PEARSON, M. D. F. R. S. &c. &c. *with a Portrait from an original Painting.*

WE are sensible that an editor of the biography of living persons should consider himself as in a very delicate situation, and the more respectable the character who is his subject, the greater should be his care ; for utterly unmerited eulogy, from motives of interest or vanity, is so usual, that any praise at all, or at least exceeding what is already generally conferred and confessedly due, must be offensive to a man of intrinsic merit. But these considerations need not restrain an editor from gratifying the curiosity of the public, who may not personally know a man of eminence, by a statement at least of the departments in which he has been occupied, and a recital of his published literary labours.

Dr. Pearson graduated at Edinburgh at a very early period of life, and of course he must have previously studied a certain number of years in that university. An extract from his inaugural dissertation, *De Putredine*, may be found  
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in the Medical Commentaries for 1775. Several communications on medical and philosophical subjects, subsequently to this period, are inserted in that annual work.

In 1784 Dr. Pearson published a treatise on the Buxton waters, principally to make known his discovery that the gas which issues from them spontaneously is not, as was believed, fixed air or carbonic acid, but phlogisticated air or azotic gas. Dr. Saunders, in his late treatise on Mineral Waters, has given due credit to the author for this discovery, as well as for his able analysis in general.

In the year 1788 Dr. Pearson published in several journals his account of the salt composed of phosphoric acid and soda, recommending it as a purgative which possesses no disagreeable taste, and is equally mild with any other of the cathartic salts. It is one of the articles in the Edinburgh Pharmacopœia of 1791, under the title of Soda Phosphorata: since that time it has been in general use.

In the Philosophical Transactions for 1791 we find Dr. Pearson's masterly and at that time unrivalled analysis of the James's Powder. Immense savings have been made by individuals since this discovery, especially in the navy, to the amount, as the late Dr. Johnson, one of the Commissioners of the Sick and Hurt Office, asserted, of several thousand pounds a year.

In the Philosophical Transactions for 1792 is a paper entitled, Experiments made with a View of decomposing Fixed Air or Carbonic Acid, by Dr. Pearson. In 1789 M. Lavoisier showed that carbonic acid might be composed by uniting carbon to oxygen: but the author in the present paper says, "The honour of the first analytical experiments on carbonic acid is due to Mr. Tennant, F. R. S." who obtained phosphoric acid and carbon by passing phosphorus through red-hot carbonate of lime, or marble powder. Whatever doubts were entertained of this fact, as stated by Mr. Tennant, they were entirely removed by the decisive experiments of Dr. Pearson.

In the Philosophical Transactions for 1794 is inserted Dr. Pearson's paper on the wax-like matter called white lack, in which he discovered a new acid called by him *laxic acid*, according to the new nomenclature of chemists.

In the same work for 1795 is Dr. Pearson's paper on the steel supposed to be made directly from the ore by the Hindoos, called *wootz*.

In the following year is our author's paper on the composition of the hard and strong metals of the antients, which he determines to have been copper united to tin, as demon-

frated by his analysis of specimens of antient metals, furnished by the president of the Royal Society the right honourable Sir Joseph Banks.

In 1797 the Philosophical Transactions contain Dr. Pearson's communication of experiments on the gas produced by passing the electric spark through water; which he determines to be the mixture of oxygen and hydrogen gas, as demonstrated by first separating the oxygen by nitrous gas to produce nitrous acid, and then inflaming the residue after adding oxygen, and thus composing water.

In the Philosophical Transactions for 1798 is published the author's paper on urinary calculi of man and brute animals; in which he shows that the sublimed acid of Scheele and the precipitate examined by the French chemists are not the same substance; and that the latter does not possess acid properties, but those of an oxide, for which he proposes the denomination *uric oxide*, a name since generally adopted.

In 1794 Dr. Pearson published the new French Chemical Nomenclature, with explanations suited to the English reader, and additions according to subsequent improvements in chemical philosophy. In a second edition of this work the author has made very considerable additions, by giving new tables of affinities, and tables of double and single elective attractions.

Among the medical dissertations and papers of the author, the prominent ones are those on the Cow-pock. The first of these, in 1798, contains a great collection of facts of this disease; and it immediately succeeded the work of Dr. Jenner, the first professed promulgator of this disease. This is neither the fit place nor time for a comparison of what has been effected by these two gentlemen; and if it were so, it would not be advisable to interfere. We honour and respect both of them as public benefactors. We have already set forth Dr. Jenner's merits in our Journal, vol. xiii., and we have given an abridged account of Dr. Pearson's late work, in 1802, in the same volume. The work entitled *The Report of the Cow-Pock Inoculation from the Practice at the Vaccine Institution in 1800, 1801, and 1802*, as read at a general meeting of the subscribers February 7th last, which we had the pleasure to hear read, will give the best account of the merit of Dr. Pearson, in conjunction with the other physicians, in establishing that institution; and carrying on the investigation of the new disease, occupied as he is in extensive practice as a physician, as a teacher on the different branches of physic, and as physician to a large London hospital. But after all it must be reserved for posterity to assign the due place of  
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of the introducers of the new beneficial inoculation in the temple of Fame; for it will be some time before the prejudices excited by jealousy and interest shall be extinguished.

Dr. Pearson was elected physician to St. George's hospital in 1787; and how serviceable he has made it to his school of medicine is best evinced by his numerous pupils, and the proficiency they make in their studies.

Dr. Pearson's elementary books on medicine are printed, and in the hands of his pupils, but not yet published.

XLIX. *Notices respecting New Books.*

*An Essay on the Relation between the Specific Gravities and the Strengths and Values of Spirituous Liquors, with Rules for the Adaptation of Mr. Gilpin's Tables to the present Standard, and Two New Tables for finding the Percentage and Concentration, when the Specific Gravity and Temperature are given. By ATKINS and Co. 4to, 1803.*

THE present work is drawn up in such a masterly and scientific manner, that we shall, as soon as we can find room, present some large extracts from it. In the mean time we insert the Preface:

“ If the subject of the following pages were regarded merely with reference to those immediate practical consequences which result from its consideration as connected with a great branch of the revenue, and still more with the interests of commerce, it must necessarily be considered as one of the highest importance. These, however, are by no means the only points of view in which it presents itself: it is intimately related to the correct appreciation of our weights and measures in general, the necessity of which appears to be universally admitted.

“ It would certainly not be expedient at this day to change those venerable standards, by which, as belonging to the first commercial nation, the traffic of the world is in a great measure guided and regulated; but it must have occurred to every man who reflects on the subject, that, in the present state of commerce and science, it would at least be convenient that we should possess some better definition of a yard-measure than that which William of Malmſbury gives us, when he states that it is the exact length of the arm of King Henry the First; and some more correct description of a

pound-weight, than that it is as heavy as 7680 grains of wheat taken from the middle of the ear\*.

“ These ideas were naturally suggested to the minds of the authors by the necessity which they have been under, in the course of their trade, of deciding on which of the various discordant authorities they should found their estimation of the weight of given measures of different fluids, and of the relation which these measures of capacity bear to the linear measures of the country; and there cannot, perhaps, be a stronger proof of the propriety of a declaratory act to settle these matters, than that there should still remain something to be assumed with respect to every one of them. This important measure is, however, as the writers of this essay believe, now in the contemplation of the government.

“ It is stated in § 16, that they have estimated the wine-gallon at 231 cubic inches, the pound avoirdupoise at 7000 grains troy, and the relation between troy weight and linear measure to be such, that the weight of a cubic inch of distilled water at 60°, when weighed in air at 60°, shall be equal to  $252\frac{1}{2}$  of the same grains.

“ The assumption with regard to the wine-gallon is founded on the established practice of the Boards of Customs and Excise to consider it as of the content which is here given; it being sufficiently known, from a very accurate experiment which was made on this subject May 25, 1688, in the presence of the Lord-Mayor, the Commissioners of Excise, Dr. Halley, Mr. Flamsteed, and other men of science, that the old wine gallon in Guildhall really contains only 224. The former, however, is, perhaps, at the present day, the legal wine-gallon.

“ The supposition with respect to the relation between avoirdupoise and troy weight, is chiefly founded on a kind of tacit agreement amongst the majority of scientific men at present to consider the avoirdupoise pound as equal to 7000 grains troy; and that which is, perhaps, of still more practical effect with regard to it,—the opinions of the best informed amongst our scale-makers: for, whether it arises from the smaller value of the articles which are weighed by the former, and its consequent less minute subdivision; from its introduction by custom rather than by statute; or from whatever other cause it may be derived, it appears to have been

\* “ Per ordinacionem totius regni Anglie fuit mensura domini regis composita, videlicet, quod denarius qui vocatur sterlingus rotundus et sine consura ponderabit triginti duo grana frumenti in medio spice. Et uncia ponderabit viginti denarios. Et duodecim uncie faciunt libram London.”

Stat. “De Ponderibus et Mensuris.” 31 Ed. I.

always subject to more uncertainty than the latter. Mr. Ward tell us, in his *Mathematics*, that he found, by a very nice experiment, that the pound avoirdupoise weighed  $6999\frac{1}{2}$  grains troy, and yet we find, from the Report on the Weights of Europe in general, in the *Memoirs of the French Academy of Sciences* for 1767, that the ratio between the avoirdupoise and troy pounds, which M. Tillet had obtained for the purpose, was that of 7004.5 to 5760.

“ The appreciation of the weight of a cubic inch of distilled water is principally founded on the experiments of Sir George Shuckburgh Evelyn, Bart., described in the *Philosophical Transactions* for 1798; from a mean of which it follows\*, that the cubic inch of distilled water at  $60^{\circ}$ , under the circumstances described in § 16, weighs 252.506 grains of the standard troy pound made for the Committee of the House of Commons in 1758, or 252.806 if weighed in vacuo.

“ A variety of motives concur to induce the authors to take these as standard experiments. All the older attempts of this kind are of no value whatever, having been made with water which was not distilled, and under other circumstances which render them liable to innumerable errors: even the celebrated experiment of Mr. Everard in 1696 was made with undistilled New River water. The only ones, perhaps, which are at all to be compared with those of Sir G. S. Evelyn are those of Dr. Robison, and the French Commissioners of Weights and Measures.

“ The former gentleman weighed a cylinder whose height and diameter were each 6 inches, taken, as he says, from a most accurate copy of the Exchequer standard, several times in distilled water at  $55^{\circ}$ , and found that it lost 42895 grains of its weight without a variation of 2 grains in the whole. Now, the solid content of the above body being  $6^3 \times .785398$ , or 169.646 cubic inches, a cubic inch of water weighs in air, according to this experiment, 252.85 grains at  $55^{\circ}$ ; or, by Mr. Gilpin's Tables, exactly  $252\frac{3}{4}$  at  $60^{\circ}$ .

“ The authors of this essay are, however, induced to prefer the experiments of Sir G. S. Evelyn to that of Dr. Robison, principally for the following reason:—He does not tell us whether his weights had been immediately and accurately compared with any of the parliamentary standards. The adjustment of the best weights is generally thus performed:—They are, in the first place, filed to something near the required weight; then ground on a stone, and afterwards po-

\* See *Philosophical Journal*, Octavo Series, vol. iv. p. 35 (No. for Jan. 1803.)

lished. Now, it very frequently happens, that some of these operations are carried a little too far, and one or two of the weights rendered somewhat too light; and when this is the case, unless the workman can be absolutely depended on, the whole set will infallibly be spoiled; for, instead of making new ones to supply their place, he will, nine times in ten, prefer the adjusting the others to them; conceiving it to be of more consequence that they should all agree, than that they should be accurately of any particular weight. When weights are successively copied from each other a number of times, the last of the series will therefore frequently be lighter than the first, and sometimes very considerably so.

“For the appreciation of the weight in troy grains of a cubic inch of distilled water at  $60^{\circ}$ , from the experiments of the French commissioners of weights and measures, we have the following data.

“We are told by the *Instruction sur les Poids & Mesures*, prefixed to M. Brissot's *Traité de Physique*; and which is certified by the commissioners to be “exactly founded on the results obtained and recorded by the said commission,” and in several other works, that they weighed distilled water in nine different cases at various temperatures, in air and in vacuo. We will, however, select the case which they have thought proper to assume as the basis of their standard of weight, viz. that in which the weight of water is taken at its maximum of density (or about the temperature of  $40^{\circ}$  of Fahr.) and in vacuo; under which circumstances they inform us that the cubic decimetre weighs 2 pounds 5 gros and 35.15 grains, or 18827.15 grains of the *poids de marc*.

“This, according to M. Tillet's appreciation of the English troy pound, which he makes equal to 7021 of these grains, is equal to 15445.7 grains troy.

“Now, we find from the report made to the Mathematical and Physical Class of the National Institute on the 27th of December 1801 \*, relative to a comparison of the metre with a scale of M. Picot's, which was exactly similar to that of Sir George Shuckburgh Evelyn, that this measure is equal to 39.38272 inches of that scale when at  $32^{\circ}$ ; or, correcting for temperature according to General Roy's table of the expansion of his brass scale, in the 75th volume of the Philosophical Transactions, equal to 39.3712 inches of the same scale when at  $60^{\circ}$ ; the French platina and iron standards, when at the freezing point, indicating the length of the metre.

“The cubic decimetre, therefore, is equal to 61.02896

\* Philosophical Magazine, vol. xii. p. 229.

cubic inches of Sir G. S. Evelyn's standard at  $60^{\circ}$ , and  $15445.7 \div 6.02896$  is equal to  $253.088$ ; the weight in troy grains of such a cubic inch of water at its maximum of density and in vacuo, according to the experiments of the French commissioners.

“Now, according to Mr. Gilpin's Tables (Phil. Trans. for 1794, p. 382), the specific gravity of water in this state is to that which it possesses at  $60^{\circ}$  as  $1000.94$  to  $1000$ . But  $1000.94 : 1000 :: 253.088 : 252.851$ . The weight of a cubic inch of water, therefore, according to this method of calculation, would, if weighed *in vacuo* at  $60^{\circ}$ , be  $= 252.85$  grains; and if weighed in air at the same temperature, under the pressure of  $29\frac{1}{2}$  inches,  $= 252.55$ ; being within  $\frac{1}{5000}$ th part of the weight deduced from Sir G. S. Evelyn's experiments.

“It was thought indispensable thus to state the hypotheses on which the authors have proceeded with respect to their calculations, and consequently with regard to the instruments which they have hitherto manufactured for ascertaining the relative strengths and values of liquors of various descriptions, and particularly those which constitute the subject of these sheets. The variation of density in alcoholic compounds is a circumstance which is not only of importance to the merchant, the spirit dealer, and the revenue officer; to the philosopher, who is engaged in the investigation of the nature and progression of those forces by which the constituent particles of matter are connected with each other, it must also afford matter of interesting contemplation. It is, therefore, of no inconsiderable consequence that every thing relating to it should be established on the best authorities and the surest foundations, and that the public should be enabled to judge of the degree of credit which is to be given to those who endeavour to elucidate so important a subject.”

### L. *Proceedings of Learned Societies.*

#### GALVANIC SOCIETY, PARIS.

THIS society has organized a commission of experiments. Thouret is elected president; Aboville, vice-president; and Izarn, secretary.

M. Winckler gave an extract of several observations on cases of deafness cured by M. Schaub, of Cassel.

The senator Abrial communicated a memoir of M. Pfingsten, director of the institution for deaf and dumb at Kiel,

tending to prove that Galvanism is of very little use in deafness.

M. Mojon gave an account of some experiments which seem to prove that Galvanism is proper for retarding the putridity of animal matters.

M. Nauche the president, and Pajot-Laforêt, communicated a new Galvanic phænomenon. Having subjected to the action of the pile frogs exposed to a temperature of ten degrees below zero, they observed, that repeated contact of the conductor communicating with the copper pole produces the development of a whitish mucous fluid, exceedingly abundant in the liver, lungs, and particularly the nerves and the heart; not very abundant in the muscles, the intestines, and none in the integuments; while the conductor in communication with the zinc pole produces no development of this fluid, and seems rather to cause that which has been produced to disappear.

C. Gautherot has made a series of experiments tending to prove that electricity is developed in the ratio of the surfaces.

C. Nauche the president, with his fellow-labourers Bonnet and Pajot-Laforêt, has been able, by means of two *homogeneous* metallic conductors, to draw off the electric fluid from the brain and spinal marrow of an ox recently killed, and to convey it to the thighs of a frog, where it produced muscular contractions. This operation succeeded also in the palpitating muscles, and could be continued only a quarter of an hour after death. The senator Lamartillière gave an explanation of the disengagement of mucous matter by the poles, and showed that it arises from a chemical decomposition.

C. Izarn gave an account of the construction of a pile invented by C. Alizeau, in which, instead of disks moistened with a saline solution, a stratum of moistened salt is employed, and which can produce its effects for a month without being cleaned.

The Commission of Medical Application, consisting of Guillotin, Dudaumon, Petit-Radel, &c. have made a great number of experiments on asphyxia by strangulation.

The application of Galvanism to diseases, suspended in consequence of the winter season, is going to be resumed at a place destined for the purpose in the School of Medicine, and in the private laboratory of the society.

LI. *Intelligence and Miscellaneous Articles.*

## ANTIQUITIES.

GENERAL REYNIER having presented to the French National Institute a tunic and the remains of some antient vestments found near Sakara, in Egypt, the three classes appointed commissioners to give in a report on these curious articles. A very exact idea may be given of this tunic by comparing it to the tunics worn by the deacons and subdeacons of the Roman catholic church, if we suppose the latter to have long close sleeves, as was formerly the case. It is ornamented with embroidered pieces, some of which descend from the shoulders; others are applied to the shoulders, and at the bottom before and behind: two pieces of the same kind surround the extremity of the sleeves. The colour of the stuff is a souci yellow, and the embroidery is puce colour, or dark brown. The design is of little importance, and has no relation either to natural objects, to hieroglyphics, or to written characters. The stuff has been wove in the loom, but the embroidery seems to have been executed according to the process of tapestry *au petit point*. In regard to the nature of them, chemists have found that the yellow stuff of the tunic is animal matter. In regard to the embroidery, the yellow tissue or ground is vegetable matter, and the brown thread, animal matter.

General Reynier was not able to procure any further information from the inhabitants of Sakara, who sold him this tunic, except that they had found it with other articles in a pit filled with sand which they had dug up.

No particular account can be given of the time when this tissue was wove, nor of the personage to whom it belonged. In the report made on it to the Institute, the author shows that it belonged neither to a Macedonian nor a Greek established in Egypt, as it has sleeves which descend to the wrist, whereas the Grecian tunic had no sleeves, or sleeves so short that they reached only to the elbow; but it appears certain that the Macedonian tunic was not different from the Grecian. The Macedonians were distinguished from the other Greeks only by their head-dress (*causia*) and their chlamys. The tunic of Sakara belonged then to an Egyptian. But the point is to determine at what period. The author of the report only says, that it cannot be older than the time when Thebes was abandoned. The grottoes of Sakara, which are  
at

at the distance of a few miles from Thebes, near the ruins of Thebes, were no doubt dug about that time. It was in the sixth century before the Christian æra that Cambyfes ravaged Thebes, and plundered it of its riches and monuments. The most remote period, therefore, which can be assigned to the time when the tunics were wove is the fifth or fourth century before Augustus.

It is impossible to say any thing more correct in regard to the personage who wore it. We are told by Herodotus that the Egyptian priests wore a single vestment of linen, and shoes of papyrus. The priests of Isis at Rome were called the troop clothed in linen, *linigera turba*. Pythagoras, who imitated them, made no use of woven stuffs made from animal matters. The tunic of Sakara was therefore not worn by an Egyptian of the sacerdotal order.

It did not form part of a female dress, for we are told also by the father of history that the Egyptian women were clothed in linen. The men used also similar vestments; but they wore above these tunics white woollen vestments. "But," Herodotus adds, "they do not wear woollen vestments in the temples, and they do not bury their dead in dresses of that kind: they would be considered as indecent."

The yellow colour of the tunic, if not the effect of antiquity, would be no proof of its not having belonged to an Egyptian; but if it be the effect of art, we may suppose that it was the distinguishing mark of some dignity. In a word, the only thing certain which can be said of this Egyptian vestment is, that it was not conveyed with the body of a deceased person to the grottoes of Sakara, which served as tombs, because it was repugnant to the ideas of the Egyptians to be interred in wove woollen cloth. It must therefore have been deposited with other riches which the proprietors wished to secure from the rapacity of the enemy.

C. Mongez has communicated to the Institute a memoir on some antient medals and other articles discovered near Aurillac, in the department of Cantal. Some workmen digging in a meadow found the portion of a circular inclosure consisting of a double wall. It was about six feet in diameter, and four feet eight inches in height; but rose very little above the surface of the meadow. The interior wall was of baked brick, exceedingly beautiful, circular, and with the joinings tending to the centre. This interior wall was surrounded by another of dry stones, destined, no doubt, to support the earth. No traces of a covering appeared, and it is probable there never was any.

In digging in this inclosure, and demolishing it, there were  
first

first found medals of gilt bronze of all the Roman emperors down to Commodus, Otho excepted; which proves that in the second century bronze medals of that prince were exceedingly rare, and that they had not then, perhaps, been carried into Gaul. Along with the small vessels of baked earth which contained these medals, there were dug up some smaller ones which contained aromatic substances. The workmen took them for tobacco; but finding by the taste that they were deceived, they threw them away, and carefully washed the vessels which contained them; so that neither the nature of these aromatics, nor even the odour which the vessels would have retained, could be known. Two small bits of very white moulded clay were also dug up: one represented a dog, the legs of which were broken; the other was a female bust, but the remainder of the body was destroyed by the digging. A clasp of bronze, and a glass ring of so large a size that it could be worn only on the thumb if it served for a ring, were also dug up.

The circular form of the inclosure discovered near Aurillac induces C. Mongez to conjecture that it might have been used for burning dead bodies, and that it was an *ustrinum*, such as the circular inclosure of earth in which the body of Augustus was burnt, and which was religiously preserved near his mausoleum, still in part existing; and such as the inclosure of the same form discovered in 1763 near Placentia, in the ruins of the antient Veleia, which appears to have been buried by the fall of a mountain, and which Winckelmann found to be an *ustrinum*. Some of the inscriptions on antient sepulchral stones of the Romans announce an express prohibition to construct an *ustrinum* near a monument. The reason of this prohibition has not yet been examined. After mentioning the law of the twelve tables, which forbade the burning of bodies nearer any edifice than the distance of sixty feet, without the owner's consent, C. Mongez is of opinion that this prohibition supposed some edifice to be in the neighbourhood, the proprietor of which insisted on the rigorous execution of the law.

In regard to the small female bust found near Aurillac, C. Mongez observes, that Montfaucon has given engravings of four female figures of the like kind. They were all of that kind of argillaceous earth called tobacco-pipe clay; the workmanship was coarse, and they had all been moulded: one of them was found in 1710, in digging in the abbey of Saint-Lomer, at Blois. It was deposited in a small grotto containing the half-burnt bones of animals, among which were found the thigh-bone of a horse and the tooth of a dog. The Gauls  
were

were accustomed to throw into the funeral piles of their dead, those animals they had been fondest of, such as dogs and horses. There is reason, therefore, to conclude that the grotto of Blois was a burying-place of the Gauls, and, by analogy, that the inclosure of Aurillac belonged to the same nation. The female bust found there exhibits the same characters as the figures published by Montfaucon; and besides, there was found along with it a figure of a dog of the same substance and workmanship.

These female figures have been so often dug up in the sepulchres of the Gauls, and the style, the workmanship, and materials have so great a resemblance, that one cannot help supposing that they must have been deposited there from the same motives. C. Mongez is of opinion that they may have represented the principal mother goddesses in general, and those in particular whom the deceased whose ashes reposed in these tombs had adopted as their protectors. A great deal has been written on the mother goddesses mentioned in the sepulchral inscriptions of the Romans: *Diis matribus . . . . . matronis*, &c. As bas-reliefs representing three females, sometimes standing and sometimes sitting, holding fruits, cones of the fir-tree, and cornucopias, were sometimes added to such inscriptions; the mother goddesses were at first taken for rural deities: but one of these monuments was found in the city of Lyons, and on others they are called the mothers of Galicia, the mothers of the Gabii, &c. Their protection therefore was extended to cities and provinces also. Keysser thinks that they were those druid females for whom the Gauls entertained so great a veneration; but this opinion is contradicted by monuments of the same kind, consecrated in countries at a very great distance from Gaul. Others considered the mother goddesses as the three Parcæ or Fates; but it is not certain that the Parcæ ever formed a part of all the nations among whom the deities in question were worshipped: besides, the latter had their proper denomination, *Fata*. In the last place, Barrier proposed a still more probable opinion respecting the mother goddesses: he supposed them to be deities common to several nations, and that their surnames denoted the places where they were worshipped.

To this may be added, that the women acknowledged them as their special protectors; for we read on two inscriptions: *Matronis Gabiabus, . . . . . Junonibus Gabiabus*. Every woman believed that she had a female genius who protected her, and whom she called her Juno. The Greeks, the Cretans in particular, and the Sicilians, rendered worship to

the celestial bears under the name of Mothers. But it appears doubtful whether the mother goddesses of the Gauls of Britain, Germany, Spain, &c. were the same deities. It is at least certain that in consequence of migrations, the notions respecting these goddesses, their functions and attributes, had at that time been strangely altered. The figures given by Montfaucon hold children in their arms, like the female figures seen on medals of some of the empresses, with the legend *Juno Lucina*; which confirms the opinion of C. Mongez, that these figures represent the mother goddesses in general, and in particular the Junos and Genii of women.

#### PALLADIUM, OR NEW SILVER.

We have just been favoured with a specimen of a metal, said to be a new one, to which the above name has been given, and which, among others, is said to possess the following properties:—1. It dissolves in pure spirit of nitre, and makes a dark red solution. 2. Green vitriol throws it down in a state of a regulus from this solution, as it always does gold from *aqua regia*. 3. If you evaporate the solution you get a red calx, that dissolves in spirit of salt or other acids. 4. It is thrown down by quicksilver, and by all the metals but gold, platina, and silver. 5. Its specific gravity by hammering was only 11.3; but by flattening, as much as 11.8. 6. In a common fire it tarnishes a little, and turns blue; but comes bright again, like other noble metals, when stronger heated. 7. The greatest heat of a blacksmith's fire would hardly melt it. 8. But if you touch it while hot with a small bit of sulphur, it runs as easily as zinc.—It is sold only by Mr. Forster, at No. 26, Gerrard-street, Soho, London, in samples of five shillings, half a guinea, and one guinea each.—We have not had time to subject the specimen sent to us to any chemical tests. When we do, we shall report the result to our readers. Where or how it has been procured we have not yet learnt.

#### IMPROVED LAMPS.

Mr. Paul, of Geneva, now in London, has made some important improvements in the construction of lamps and reflectors, by which, among other advantages, that light which is usually thrown up into the atmosphere and lost, is reflected to the space intended to be illuminated; by which a much greater effect is produced, and at the same time a saving in the consumption of the oil. We understand they are soon to be employed by Mr. Smethurst in lighting the streets.

## ASTRONOMY.

In our last we presented our readers with tables containing the geocentric motion of the two new planets, Ceres Ferdinanda and Pallas, for the month of April; we now subjoin tables of their motion calculated for the month of May 1803.

Geocentric Motion of Pallas.					Geocentric Motion of Ceres Ferdinanda.				
	Right Ascension.			Declin. North.		Right Ascension.			Declin. South.
May 3	18 <sup>h</sup>	55 <sup>m</sup>	15 <sup>s</sup>	18° 38'	19 <sup>h</sup>	11 <sup>m</sup>	54 <sup>s</sup>	24° 6'	
6	18	55	4	19 7	19	12	28	24 15	
9	18	54	42	19 35	19	12	50	24 25	
12	18	54	8	20 1	19	12	59	24 35	
15	18	53	22	20 26	19	12	52	24 46	
18	18	52	24	20 51	19	12	30	24 57	
21	18	51	15	21 14	19	11	56	25 9	
24	18	49	56	21 36	19	11	5	25 22	
27	18	48	27	21 55	19	10	3	25 35	
30	18	46	49	22 14	19	8	44	25 48	

## GALVANISM.

Professor Aldini, when on his way to France, made the following experiment at Calais:—He extended a wire from the top of the West Jetty to Fort Rouge, distant about 160 feet, and which are separated by an arm of the sea. The extremity of this wire towards the jetty was immersed in the water, the other extremity was fixed to a mast, and a portion of the wire was free. A second wire was disposed in such a manner, that its lower extremity was immersed in the water at the bottom of the mast, while its upper extremity could be made to communicate with the wire fixed to the mast. Between the extremity of the wire attached to the mast and that of the second wire immersed in the sea the professor placed an animal, as a prepared frog for example, which being then brought into contact with the two wires, immediately experienced the usual contractions. The sea water, therefore, served as a conductor between the other extremities of the wires.

## A NEW SPECIES OF INOCULATION.

Accounts have been received from one of the travellers in Africa (Hornemann, we believe), stating, that in a district which he has visited he found a kind of inoculation practised, which answers the same end respecting syphilis that the vaccine pock does respecting the small-pox—exempting the patient from the possibility of infection.

LII. *Letter from Dr. OLBERS of Bremen to Baron VON ZACH, on the Stones which have fallen from the Heavens* \*.

YOU know that in a lecture which I delivered in the Museum of Bremen in the year 1795, on the shower of stones, as it is called, which fell at Siena in Italy, I expressed the same idea which I lately read with much satisfaction in a letter of Laplace, in which he says: “It is not impossible that large masses, detached from some of the celestial bodies, and particularly from the moon, may have sometimes been projected to the earth.” As you require a more circumstantial illustration of this subject, I embrace the present opportunity of gratifying your wishes.

I must readily acknowledge, that when I wrote the before-mentioned essay on the shower of stones which fell at Siena, I considered these stones to be of a volcanic origin. The stones found at Siena fell at the distance of sixty miles from Vesuvius, only eighteen hours after the commencement of the great eruption which destroyed the unfortunate Torre del Greco, and of which so elegant a description has been given by sir William Hamilton. I was acquainted with the principles of Zolner and Lichtenberg, according to which these stones could not be ascribed to Vesuvius; but to me these principles did not appear satisfactory. The great velocity which would be necessary to make a mass projected from Vesuvius to proceed to so great a distance, was not sufficient to induce me to abandon my opinion. By calculation I found that it was not so great as what might be supposed to be produced by the dreadful convulsion which takes place on such occasions. What tended chiefly to confirm me in my idea was, that sir William Hamilton then believed that he had often found at Vesuvius stones which had a great resemblance to those which fell at Siena †. I also supposed that Vesuvius might have projected from one of its mouths a half-fused mass at an angle of from forty to fifty degrees, which, like every thing else proceeding from the crater, was in a high degree electric; that this mass had

\* From *Algemeene Konst en Letter-Bode*, No. 17, 1803.

† Stones of the same nature, at least as far as the eye can judge of them, are frequently found on Mount Vesuvius; and when I was on the mountain lately I searched for such stones near the new mouths; but as the soil around them has been covered with a thick bed of ashes, whatever was thrown up during the force of the eruption lies buried under those ashes, &c.—*Philosophical Transactions* 1775, p. 104.

taken its direction towards Siena; that when within a certain distance of the earth it had discharged itself; and that in consequence of this discharge it had burst, so that the fragments fell down in the state of ignited stones\*.

On the other hand, however, it is known that no such stones are found on Mount Vesuvius. It has been lately shown by the mineralogical description of Count de Bournon, and the chemical analysis of Mr. Howard, that the stones found at Siena have a perfect resemblance to all the other stones which have been seen to fall from the heavens, and which certainly cannot be productions of our earth†. My former explanation and conjectures fall therefore to the ground, and we must class the stones of Siena among those which are formed by nature in some mode with which we are unacquainted, and which bursting always with the appearance of light fall down to the earth.

Since the time when naturalists first ventured, or were obliged, to believe in the falling of such stones, it has been found that this phænomenon is not so uncommon as was supposed. During the last century, fourteen or fifteen instances at least can be quoted.

When I erroneously ascribed in my lectures the stones found at Siena to an eruption of Vesuvius, the nature of other stones fallen from the heavens, which certainly were not of volcanic origin, had given occasion to the question, whether it was impossible that heavy masses could be projected to the earth from other heavenly bodies, and particularly from the moon? The celebrated stone which fell near Argos Potamos, in the year 462 before Christ‡, induced me in particular to undertake an examination of this question; and on this occasion my learned friend, Mr. Bredenkamp, clergyman of Bremen, was so kind as to collect for me all the passages in antient authors which make mention of this stone. In regard to the rest, the reader may consult what Struyk, Pingré§ and Chladni|| have said on the subject.

It appears from calculation, that when a greater vertical velocity than 34435·7 Paris feet in a second can be given to a heavy body projected from the earth's surface, supposing the atmosphere to oppose no resistance, this body would not

\* The abbé Tata actually saw a large fire-ball thrown up from Vesuvius, which when it had reached a certain distance burst, and the abbé then heard a noise like that of stones falling down.

† Howard's Experiments and Observations on certain Stony and Metalline Substances, &c.—Philosophical Transactions 1802.

‡ Plin. Hist. Nat. lib. ii. cap. 58.

§ Cometographie, p. 256.

|| Philosophical Magazine, vol. ii.

fall back to the earth, but would continue to move from it *in infinitum*. A cannon bullet sometimes acquires a velocity of 1800 or 2000 feet in a second\*; and heavy masses are projected from the volcanoes of our earth with a velocity four or five times as great. This velocity, therefore, is far greater than that of 35,000 feet per second. But even if it were possible, by the help of chemistry and mechanics, to give a ball a velocity of 35,000 feet, or even more, per second, the strong resistance opposed by the air, which always increases at the least as the square of the velocity, would tend very much to destroy it. We may therefore consider it as almost impossible that heavy bodies can ever be entirely projected from the earth. I consider it altogether unnecessary to take into account in this calculation the influence which the attractive force of the moon would have on the motion of a body projected in this manner.

But the case is entirely different if we subject to calculation the motion of a heavy body projected from the surface of the moon. The moon is a great deal smaller than the earth, possesses a much weaker power of attraction, and at the same time its atmosphere is of so small extent, and so rare, that it could oppose very little resistance to a heavy body in a state of motion. It is therefore possible that a heavy body might be projected from the moon with such a velocity as to carry it to a distance where it would be attracted with greater force by the earth.

It is found by calculation that 7780 feet per second is the minimum of velocity with which a body projected from the moon would require to fall upon the earth. It appears, therefore, that if heavy bodies were projected from the moon with a vertical velocity of 7800 or 8000 feet under certain circumstances, some of these bodies would reach the earth and fall down upon it. Such a velocity appears to me to be very possible. The surface of the moon, by the new craters which arise, shows evident traces of violent eruptions, by which even as great or greater velocity might be communicated to the bodies thrown up by them.

It appears then, that it is not altogether impossible that the stones or masses which have been seen to fall from the heavens, and which, though entirely different from all the mineral bodies of our earth, have a great resemblance to each other, may have fallen from the moon. It is in their great similarity and correspondence that the grounds of this opinion are to be found; for the similarity of their external appear-

\* Mém. de l'Acad. Royale des Sciences 1769, p. 247.

ance, and of their component parts, evidently show that they have all had the same origin. If we admit, with Halley and Chladni, that, besides the large celestial bodies, there are in boundless space an infinite number of small masses which move about till they approach so near a planet as to enter its atmosphere, when they take fire, burst, and fall down upon it; we can explain why all these masses dispersed throughout infinite space consist of iron, nickel, siliceous earth, and talc earth, which, according to the ingenious experiments of Mr. Howard, are the component parts of all the masses which have fallen from the heavens.

But, on the other hand, great difficulties occur, if these stones which have fallen from the heavens be seriously considered as bodies projected from the moon. In consequence of the motion of the moon, bodies projected from it acquire, besides their projectile velocity, that velocity which the moon has in the direction of the tangents to her orbit. If we therefore take this velocity into the account, it will appear, that heavy bodies projected from the moon with a velocity of 8000 feet or more per second, as soon as they have got to such a distance from the moon as to experience less attraction from it than from the earth, would describe around the earth a conic section passing more or less through the moon. These conic sections, according to the difference of the direction and velocity, may be either hyperbolas or ellipses\*. In order that the body should fall to the earth, the ellipse it describes ought to be of such a nature that its perigeum falls within the body of the earth, or at least within its atmosphere. The moon, therefore, must perceptibly decrease in size, as it would be necessary that it should throw out a great number of masses to make some of them reach the earth. For this purpose also a very limited direction and velocity of the heavy bodies would be required. And would not an infinite number of such small fragments move around our earth as satellites? Would they not be visible through our best telescopes, as we know that fire-balls sometimes are of very great size, and as the observation of Ceres and Pallas has shown that bodies of a very small diameter become visible to us when illuminated by the light of the sun; or are the shooting stars, which seem to have a cosmical origin, such small satellites of our earth? Perhaps we ought to class among these also the small pale luminous spot which Schröeter saw pass over the field of his telescope. These objections, and others which

\* A prodigious velocity would be required to make a body projected from the moon to fall to the earth in an hyperbola.

arise from an examination of the circumstances of their falling, seem to me to be of great importance, and difficult to be removed.

I am therefore far from asserting that these stones which have fallen to the earth are to be considered as masses projected from the moon; nor does Laplace make any such assertion. His object, as well as mine, is to show philosophers engaged in researches on this subject the possibility of these masses having a *jelenitic* origin. It is much to be wished that the ingenious Chladni would favour us with a new edition of his celebrated essay on the mass of iron found in Siberia \*, as he no doubt would be able, from Benzenberg and Brandes' Observations on falling Stars, Howard's Chemical Examination, and from various other documents, to make considerable additions to it.

LIII. *On the Nervous Power, and its Mode of acting* †.

IT has been generally supposed by physiologists that the power by which sensations were transmitted to the sensorium, was likewise the medium by which mental impressions were communicated to the body. But this opinion is totally inconsistent with that most frequent observation, that very often every possible susceptibility of sensation is lost in a whole limb, which nevertheless retains voluntary motion. This appears most strikingly in the St. Vitus's dance, which deprives the afflicted parts of every susceptibility of sensation without destroying voluntary motion. And, on the contrary, there are cases where the functions of the senses remain unimpaired, and are sometimes increased, while the muscular motion is completely lost. In order to explain this phenomenon two kinds of nerves have been hypothetically adopted, nerves of sensation and nerves of motion; but without rendering the explanation more satisfactory. Certain it is, that the action of mental irritation upon the body, and the communication of the sensations with the sensorium, are effects of two different powers, which cannot possibly have their seats in one and the same organ.

That which is understood by the word *nerves* consists properly of two entirely different parts, the *medullary substance*

\* Über den Ursprung der von Pallas gefundenen und anderer ihr ähnlicher Eisenmassen, und über einige damit in Verbindung stehende Naturerscheinungen; Von E. T. T. Chladni. Riga 1794. 4to.

† From *Reil's Archiv für die Physiologie*, vol. i.

of the nerves, and the *cellular texture*; the latter being composed of cylindric tubes, containing the former in their cavity: and in these tubes, these sheaths of the nerves, is the seat of the power which continues the irritation of the mind into the body. The medullary substance of the nerves, on the other hand, possesses no other power than that of exciting in the sensorium sensations corresponding with the impressions received by the senses. The cellular texture of the nerves possesses a power of contraction, elasticity; but the medullary substance of the nerves possesses only the simple sensitive power of the nerves. From this, the above-mentioned phænomena, especially those observable in St. Vitus's dance, are easily explained. The sensitive power of the nerves disappears, because its organ, the medullary substance, is destroyed or wounded. The two following arguments confirm the correctness of this opinion:—1. According to Aaneman's numerous experiments on the regeneration of the nerves, each end of a nerve which has been cut in two forms itself into a scirrhous, and both afterwards join again in the cellular texture; but the lower end of the nerve thus intersected always loses its sensitive power, whereas it recovers in a few months the power of motion when both the ends are joined again by a sufficient quantity of the cellular substance. 2. Arsenic and all sorts of mercurial preparations, when immediately applied to the brain, exhibit not the least immediate effect upon it. But those very minerals, when applied in any other part, excite the most violent convulsions of the whole body; and it is well known, from other circumstances, that these produce the greatest irritation upon the cellular texture. Without entering into any further elucidation, every one will easily conceive the great importance of these ideas in the explanation of many physiological and pathological phænomena.

LIV. *Letter from Dr. BARTON to Professor ZIMMERMANN, on the fascinating Faculty which has been ascribed to the Rattlesnake, and other American Serpents.*

[Concluded from p. 202.]

AFTER quoting Mr. Michaëlis's observations, which I have already noticed, Mr. Blumenbach has the following words:—"I would, however, add another mode of explanation from Dr. Barton's work itself, where he assures us that the result of his inquiries, whether the rattlesnake creeps up  
trees

trees or not, induces him to believe the latter to be the case. The rattlesnake," Mr. Blumenbach continues, "is also one of the laziest of all the serpent tribe; under these circumstances it seems very natural that such a lazy animal should be endowed with the fascinating power of bringing down from trees small animals, which otherwise would have nothing to fear from a snake that cannot creep upwards."

It is true, as I have asserted, that the rattlesnake is one of the most sluggish species of serpents; and further inquiries have confirmed me in my former opinion, that this serpent does not climb up trees. But if Mr. Blumenbach had recollected what I have so particularly said on the subject of the nidification of our birds\*, he would not, I presume, have imagined that it is at all necessary that the serpent should be endowed with "a fascinating power of bringing down from trees small animals." I have shown that the rattlesnake feeds upon bull-frogs, which are never found upon trees; upon the ground-squirrel, which is most commonly found upon the ground; and upon the *fringilla erythrophthalma*, or ground-robin, a species of finch, which receives its common English name from its being so generally seen upon the ground†. I may now add, that this reptile feeds upon young rabbits (*lepus americanus*), wild mice of different kinds, moles (*forex aquaticus*), and many other small animals, which it cannot have much trouble in obtaining without its possessing the power of charming.

But although the rattlesnake is, in reality, a sluggish reptile, it is nevertheless an animal of more activity than Mr. Blumenbach seems to imagine. I believe it is true, as the old Indian‡ informed Mr. Heckewelder, that in the spring season these reptiles make considerable journeys from their dens in search of food. I know it to be a fact, that they swim across streams of water, and even over large rivers. It is observed by the hunters and others, that the rattlesnake swims faster than it moves on the land. Indeed, it seems to run upon the surface of the water. It is very probable that in these watery peregrinations it may occasionally have opportunities of catching certain kinds of fish. But this is a mere conjecture, which at present I am incapable of supporting by any decided fact.

I have observed in my memoir, that "among the Indians of South America I do not find any traces of the notion that serpents can fascinate other animals." Mr. Blumenbach,

A Memoir, &c. pages 50, 51, 52, 53. † Ibid. pages 63 and 64.

‡ See page 31.

however, informs us, that Dobrishoffer asserts, in his History of the Abipons, “that all the Spaniards and Indians in that part of Paraguay unanimously ascribe a like property to the snake called *ampalabas*.” I also find that Dr. Bancroft has mentioned the fascinating power of a large but innocuous species of serpent which inhabits Guiana\*.

I was ignorant of these facts when I printed my memoir; and now that they are known to me, they do not appear to be of much consequence. They certainly do not prove that serpents are endowed with the power of fascinating. It is not by any means ascertained that the Abipons have not derived the notion from the Europeans, with whom they have been long acquainted.

Since the publication of my memoir I have been able to make a more complete collection of the sentiments of the North American Indians on the subject. I am led to believe, that it is far from being the general opinion among these people that the rattlesnake is endowed with the faculty of charming. I cannot in any other way so strikingly show the notions of the Indians on the subject, as by extracting part of a very curious manuscript which I received from my friend Mr. John Heckewelder:—“Having questioned Indians a number of times with respect to snakes having the power of charming, and always being answered in the negative, I was at length desired,” says Mr. Heckewelder, “to give the reason the white people had for believing such a thing; which not being satisfactory, Pemaholend† declared: ‘The rattlesnake obtains its food merely by slyness, and a persevering patience. It knoweth as well where to watch for its prey as a cat does, and succeeds as well. It has, and retains, its hunting grounds. In spring, when the warm weather sets in, and the woods seem alive with the smaller animals, it leaves its den. It will cross a river and go a mile and further from its den to the place it intends to spend the summer; and in fall, when all the young animals bred this season are become strong and active, so that they are no more so easily overtaken or caught, it directs its course back again, to its den, the same as a hunter does to his camp.

“‘The white people,’ continued Pemaholend, ‘probably have taken the idea of this snake having the power of charming from a tradition of ours (the Indians), which our forefathers have handed down to us, from many hundred years

\* An Essay on the Natural History of Guiana, &c. p. 205: London 1769. Mr. Stedman, a late writer, positively denies the existence of this fascinating power in the aboma, the serpent mentioned by Dr. Bancroft.

† An aged and much respected Delaware Indian.

back, and long before ever the white people came into this country. Then (they tell us) there *was* such a snake, and a rattlesnake too; but then there was only *this one* snake which had this power, and he was afterwards destroyed; and since that time it hath never been said that any other of the kind had made its appearance.”

The whole of this tradition, as related by Pemaholend, is in my possession. It is a very curious piece of American mythology, and will be published at large in another place, perhaps in my Fragments of the Natural History of Pennsylvania. It is a new proof of my assertion, “that the mythology, or superstitious religion, of the Americans is a fragment of that mythology whose range in Asia and in Africa has been so extensive\*.” But this tradition is interesting in the discussion of the question in which I am now engaged. It plainly shows that the Indians do not in general suppose that the rattlesnake is gifted with the faculty of charming; and it renders it still more doubtful that the whites derived this notion from the Indians.

The Indians are so far from believing in the existence of this faculty in snakes, that the worthy gentleman from whom I received the tradition which I have just mentioned, assures me that he would be unwilling in future to trouble them with any further inquiries on the subject, as the sure reward of the pains of inquiring is a laugh at the easy credulity of the Whites. I may here add, what I have mentioned in my memoir, that Mr. William Bartram never understood that the nations of Indians among whom he travelled had any idea of the fascinating power of snakes†. Among other Indians Mr. Bartram visited, with the zeal and knowledge of a naturalist, the nations of East and West Florida. As this ingenious and amiable gentleman believes that serpents can charm other animals, there will be no suspicion, among candid people, that he has concealed the opinions of the Indians on the subject.

I think I have now considered every essential part of Mr. Blumenbach's remarks on my memoir. I have little doubt that this illustrious professor, who is not less candid than he is learned and ingenious, will give to the new facts which I have adduced all the consideration which they merit. What is the proportion of that consideration I must leave it with you and other able judges to decide. In the meanwhile I am not a little flattered that one of my earliest essays in natural history has solicited so much of the attention of the philosophers of Europe.

\* A Memoir, &c. p. 16—Note.

† Ibid: p. 14.

You inform me, that my explanation of the supposed fascinating faculty of serpents is adopted by many of your naturalists. This is pleasing to me. Indeed, such is our selfishness, that I fear I should have felt somewhat gratified to learn that the theory had been adopted, though I myself had relinquished it. But I assure you that, as yet, I have seen no cause to relinquish it. On the contrary, I possess a great body of additional facts in support of it. These facts will be carefully adduced in that part of my Fragments which is intended to comprise the history of the *amphibia* of Pennsylvania.

In my native country the explanation which I have offered has been adopted by many persons. But there are others who still believe in the existence of a true fascinating faculty in the rattlesnake and other serpents. What change time and further attention to the subject may accomplish in them, I know not. But why should we expect to make all philosophers converts to our opinions? Almost every phenomenon, almost every fact in nature, seems to admit of an explanation upon more than one principle. The stream of inquiry is often diverted by trifling circumstances into very opposite directions. Prejudices, or, to name them by a milder phrase, the earlier biases of our minds, frequently detain us, in the investigations of science, in a long and pleasing reign of tyranny. Our first love is said to be the strongest. Our first principles in science, in religion, and in politics, are often adhered to with the extreme of pertinacity. He who, turned of fifty years of age, relinquishes a favourite error, has infinitely more merit than the world may be willing to allow him.

I beg you, sir, to make what use of this letter you may think proper. If it shall add any thing to the stock of your knowledge on the subject, or if it shall serve to amuse you in an hour of leisure, I shall feel highly gratified.

Be assured that I am, with very great respect,

Dear sir, your faithful and obliged  
servant and friend,

BENJAMIN SMITH BARTON.

#### POSTSCRIPT.

I BEGAN this long letter on the very day that I first saw Mr. Blumenbach's remarks in Tilloch's Magazine. I had not finished it before I received a copy of the original publication of the professor. In the conclusion I find he urges me to extend my inquiries into the real use of the *crepitaculum caudæ*, or rattle of the crotalus. This is certainly a question worthy

worthy of the attention of American naturalists, who enjoy the best opportunities of investigating the subject. I am, indeed, inclined to think that we are not yet acquainted with the real or exclusive use of the rattle of the crotalus. That it was given to this reptile to warn man and other animals of a dangerous enemy, does not appear a sufficient explanation of the use of the organ. Many serpents whose poison is not less deleterious than that of the rattlesnake, are entirely destitute of any apparatus like the crepitaculum of this reptile. Besides, we have seen that, when most intent upon obtaining his prey, the rattlesnake keeps his rattle still. This would seem to show that it was not designed to terrify its enemies. Indeed, it is highly probable that one reason why the rattlesnake so frequently succeeds in capturing animals is, because he makes no noise, and therefore surprises his prey.

I do not doubt that the crepitaculum of the rattlesnake is an organ of very essential importance: but I must say that it has always been deemed of more importance than it can yet be shown to be. Thus it has been said to give an unerring indication of the age of the reptile. In this respect it is a less sure criterion than has been generally imagined. It is a fact which, I believe, has entirely escaped the notice of all the writers on the natural history of this reptile, that the rattles are formed before the exclusion of the young ones from the uterus. Towards the latter end of August a number of female rattlesnakes were opened. The young animals were about five inches long, and about the thickness of a small-sized goose-quill: the scales were formed so as to be visible to the naked eye; the head very large, and the fangs, though of a somewhat gelatinous consistence, were shaped, and distinctly visible. The rattles were so far formed that three bells could be plainly discerned; and this was the case in more than sixty instances. Hence it is evident, that when they are excluded from the womb the young animals have at least three rattles. After this, I believe, they generally acquire two bells every year. Yet, in one instance, a rattlesnake \* has been known to acquire four bells in the term of a year. I may add, that the bells are liable to be lost: they are sometimes broken, and it is not improbable that they wear out.

I am sometimes almost inclined to think, with your learned and eloquent countryman Mr. Herder, that "natural history has reaped no advantage from the philosophy of final causes." And yet without an inquiry into the ultimate intentions of nature, what is natural history? A barren waste

\* In the Museum of Mr. Peale.

of unconnected facts. Speculations, which too often, indeed, proceed from indolent philosophers, are necessary to render natural history an agreeable and even useful science. We are, moreover, sometimes able to discover the final cause or intention of nature in her operations. And as to the *usus partium*, every philosopher must confess that this is a question which ought never to be neglected. It is a kind of soul to the science of anatomy. It is exceedingly to be regretted, however, that we are so often at a loss to determine the uses of parts even where the forms and structure of these parts are well known to us.—But let us not despair. Nature intends to reveal to us many things (precious in their kind), which are now entirely unknown to us. Physiology will, in time, assume one of the highest stations in the range of sure as well as splendid sciences.

LV. *A Survey and Report of the Coasts and Central Highlands of Scotland; made by the Command of the Right Honourable the Lords Commissioners of his Majesty's Treasury in the Autumn of 1802. By THOMAS TELFORD, Civil Engineer, Edinburgh, F. R. S.*

MY LORDS,

**I**N reporting upon the survey I made in Scotland, in obedience to the instructions I had the honour of receiving from your lordships, dated the 1st of July 1802, I find the business may be most conveniently arranged under the following heads:

I. What regards rendering the intercourse of the country more perfect, by means of bridges and roads.

II. Ascertaining various circumstances relative to the Caledonian canal, especially with regard to the supplies of water on the summit level, and the best communications from this canal to the fishing locks at the back of the Isle of Skye.

III. The means of promoting the fisheries on the east and west coasts.

IV. The causes of emigration, and the means of preventing it.

V. Improving the means of intercourse between Great Britain and the northern parts of Ireland; particularly as to the bridges and roads between Carlisle and Port Patrick, and also the harbour of Port Patrick.

Under each of those heads are comprehended subjects highly deserving the attention of government; the more they

they are investigated, the more important they will appear, and the public will become more fully convinced, that the general interests of the British empire are extensively connected with the several improvements which are mentioned in your lordships' instructions.

### *Of Bridges and Roads.*

The obstacles which at present obstruct the communications in the north of Scotland are numerous and well known, not only to the inhabitants, but to every person who has travelled through, or even inquired into the state of, the country.

Previous to the year 1742, the roads were merely the tracks of black cattle and horses, intersected by numerous rapid streams, which being frequently swoln into torrents by heavy rains, rendered them dangerous or impassable. The military roads, which were formed about this time, having been laid out with other views than promoting commerce and industry, are generally in such directions, and so inconveniently steep as to be nearly unfit for the purposes of civil life; and in those parts where they are tolerably accessible, or where roads have since been formed by the inhabitants, the use of them is very much circumscribed from the want of bridges over some of the principal rivers.

The general connections of the country may be stated as leading from Edinburgh to the north and north-west counties, by means of one road through the Highlands, and by another along the east coast and south shore of the Murray Frith to the town of Inverness, and from thence through Beauley and Dingwall, to Tain in Ross-shire. From Glasgow and Greenock, the communications by land are through Argyleshire, to the western parts of Inverness and Ross-shire, and to the shores opposite the whole of the Hebrides. There is an important communication from Inverness westward, across the country to Fort Augustus and Fort William, and from Fort Augustus there are just the vestiges remaining of what was once a military road to Bernera, opposite the back of the Isle of Skye.

In considering these lines of roads, it appears most regular to begin on the borders of the improved country, and near to the seats of commerce and industry. In proceeding from Edinburgh northwards, by the east coast and Moray Frith to Inverness, or through the central Highlands to the same place, and from thence to Tain on the Dornoch Frith in Ross-shire, we find the communications intercepted, and we learn that accidents frequently happen from the want of a  
bridge

bridge over the river Tay, at Dunkeld, in Perthshire; another over the river Spey, at Fochabers, in Banffshire; a third over the river Beauley, in Inverness-shire; and a fourth over the river Conon, near Dingwall, in Ross-shire. These rivers are large, and at present are all crossed by means of ferry-boats.

At Dunkeld the river Tay is deep and broad, and there is reason to expect the foundations will be expensive, the bed of the river at and for a great distance above and below the town being composed of alluvial earth and gravel. The best situation for a bridge is a little below the lower ferry; at this place there is a straight reach of the river, and in winter the ice is broken by passing over a ford nearly opposite the mouth of the river Bran. This situation will also connect with the improved lines of road which are proposed to be made on each side of the river. It is probable that a flat rubble stone will be got near the slate quarries, which are within a short distance of the place. Freestone of a durable quality is to be had near Dundee: it may be brought by water carriage to Perth, and from thence by land to Dunkeld.

Under all these circumstances the expense would be considerable; and, taking into account the uncertainty of the foundations, the amount cannot be stated at less than 15,000 l.

The two ferries which are now at Dunkeld belong to his grace the duke of Athol; he has authorized me to state, that if government will defray one-half the expense of a bridge, he will advance the other half; that he will give up his interest in the ferries, if in lieu thereof a reasonable toll be put upon the bridge, in order to liquidate the capital advanced by the duke: after this has been accomplished, with a small surplus to answer the repairs, the bridge ever after to remain free of toll.

This seems a very reasonable and just mode of defraying the expense: the safety and accommodation would be so great, that no person travelling that road could object to paying the same toll for a safe and convenient bridge which at present is paid for a dangerous and inconvenient ferry-boat, especially when there will be a certain prospect of having a bridge free of toll in a few years.

This bridge is of the first importance to the Central Highlands; it would accommodate a great district of that country, and at the same time facilitate the communication with the North Highlands.

The river Spey is also rapid and deep, being the drain of a great extent of mountainous country, where there is much

rain. It is of course a very dangerous ferry. This ferry is on the great coast road eastward from Inverness and Fort George, through the towns and cultivated country in Murray and Banff-shires, from whence it passes through Frazerburgh and Peterhead to Aberdeen.

Another branch of road also strikes off at Fochabers, on the Spey, and passes by Huntly and Inverury to Aberdeen.

The necessity of having a bridge over the Spey at this place became so urgent, that his grace the duke of Gordon began a subscription in the adjacent country in order to raise a part of the money necessary to defray the expense of a bridge; his grace set a liberal example, and it has been followed by most of the gentlemen in that part of the country. In consequence of this exertion, a contract has been entered into, and some steps have been taken towards carrying this useful work into execution; but unless government will grant an aid equal to one-half the expense, the works must still be left unfinished, and unfit for the purposes intended.

The situation fixed upon for this bridge is adjacent to the ferry and town of Fochabers. At this place a rock passes quite through the river, at about from eight to ten feet below the surface of ordinary low water; and, as far as I could learn, it is the only place, unless at a great distance up the country, where the rock passes quite through the river. On these accounts it was judicious to prefer it for the situation of the bridge; and it was fortunate this happened precisely in the line of the present road.

As one-half of the expense will be raised by subscription in the neighbourhood, if government will defray the other half, I understand it is proposed that the bridge shall be free of toll. The expense cannot be stated at less than 12,000*l*.

I have made plans and sections for both those bridges. On account of their being so nearly connected with the seats of the two before-mentioned noblemen, I have introduced more decorations than what are absolutely necessary for common road bridges. This extra expense will not amount to much; but whatever it is, I propose it shall be defrayed by those noblemen.

I last year produced plans and estimates for the bridges over the rivers Beauley and Conon: I have again examined the proposed situations, and perceive that, on account of the uncertainty of the foundations, and the alteration which must be made in some parts of the beds of the rivers, it will be necessary to take the expenses of each bridge at 5000*l*. instead of 4000*l*.

These two bridges are greatly wanted, in order to facilitate the

the communications with Ross-shire, Sutherland, and Caithness; they are equally so for the north-west coast of the main land, and the northern parts of the Hebrides; they are the roots from which a great number of branches of roads are to proceed, which are necessary for the improvement of the country and the extension of the fisheries.

Before entering upon the consideration of the roads to the north of the line of the Caledonian canal, it is necessary to speak of the communications from Glasgow and Greenock by Argyleshire to Fort William. From Glasgow there is already a road, which passes through Dumbarton up the west side of Loch Lomond, by the upper ends of Loch Long and Loch Fyne to Inverary. From Greenock, by crossing the frith of the Clyde, there is a road which passes up the east side of Gare Loch and Loch Long, and joins the road to Inverary at the top of Loch Long. From Inverary there is one road which passes the upper end of Loch Awe, and another which crosses that loch by a ferry at Port Sonachan; and these roads unite at Bunawe, on the banks of Loch Etive. From Bunawe the road is already made to Oban, and a branch crosses Loch Etive and passes along the south side of the Linnhe Loch and Loch Eil to Fort William. Between Bunawe and Fort William there are three ferries, over arms of the sea, which running many miles into the land, cannot well be avoided: in other respects, the communication from Glasgow and Greenock, thus far, is tolerably good. The Highland Society, in the excellent report to which I shall frequently have occasion to refer, and which may be found in the appendix\*, have pointed out a new line of road from the north side of the Frith of Clyde, nearly opposite to Greenock, to be carried to the Bay of Strachan upon Loch Fyne: this would be a very direct line from Greenock to Inverary, but it would be subject to two ferries, and it seems doubtful whether this inconvenience would not overbalance the additional distance round the upper end of the lochs; at least, as there is already a very good communication by this road, it seems most prudent to attend to the other more necessary portions of road before this is undertaken.

From Fort William it will be very adviseable to improve and extend the road which passes along the north side of the portion of Loch Eil which turns from Fort William to the west, and to carry it from thence across the upper end of Loch Shiel, through Arrasaig to Morer, as described in the

\* This shall appear in our next Number.

report of the Highland Society. This would open a very direct communication from the Clyde to the fishing lochs at the back of Skye, to Skye itself, and to the islands of Egg, Rum, Muck, Barra, and South Uist. This would prove of great importance to the fisheries on account of facilitating intelligence, which is one of the most necessary steps to promote the success of this business.

In the year 1796, Mr. Brown, of Elgin, made a survey and estimate of this road from Fort William to Morer, and stated the expense at 6456l.; since that time I understand that about 1500l. has been laid out upon it, so that 4000l.; and perhaps somewhat more, is still required to render it perfect.

With regard to the improvement of the roads which lead from Fort William east to Inverness, there is one principle which in future ought never to be lost sight of, which is, to make the new roads as near to the banks of the rivers and lochs as is practicable at a reasonable expense. If the Caledonian canal is executed, it is more than probable that many improvements will be pointed out in the course of carrying on that great work; and it would be imprudent to decide rashly in a matter which is so much connected with this national object.

We now come to consider the communications of the countries which lie to the north and west of the track of the Caledonian canal. From this valley, which runs from Fort William to Inverness, it is of great importance that there should be lines of communication with the Isle of Skye and the fishing lochs which lie at the back of it; these lines of road are not only necessary for promoting the fisheries, but are urgently called for by the situation of the interior parts of the country, where there are many fertile valleys which hitherto have remained nearly inaccessible: it is incalculable the loss which the public has sustained, and are about to suffer, from the want of roads in this country.

From the bridges of Beauley and Conon, lines of road, from the same important causes, are wanted in several directions to the west coast, and through the whole of the counties of Ross-shire, Sutherland, and Caithness. The outlines of those roads are well described in the report of the Highland Society; and although there is a diversity of opinion as to the comparative importance of some of them, as well as of those to Skye, yet it is evident that they are all necessary for the welfare of the country.

The empire at large being deeply interested in those improvements, as it regards promoting the fisheries and in-

creasing the revenue and population of the kingdom, justifies government in granting aid towards making roads and bridges in a country which must otherwise remain, perhaps for ages to come, thus imperfectly connected. Yet as the land-owners in those extensive districts through which the roads would pass, and indeed the whole of the adjoining districts of country, would enjoy improved cultivation and pasturage, increased incomes, and all the blessings which are to be derived from a facility of intercourse, it is certainly just that they should contribute a share with government in the expense of acquiring those advantages. They might be enabled to do this without inconvenience to the present possessors, by being empowered by an act of parliament to sell land, or borrow money upon the land, to the amount of their proportion of the expense to be incurred by the roads and bridges. This is reasonable, because the money so raised would be applied to improve the remainder of the entailed estate, which would be enhanced in value though somewhat diminished in extent.

The expense of the lines of road which were surveyed and estimated by Mr. Brown, comprehending the whole of the beforementioned counties, and nearly 1000 miles in length, did not amount to 150,000*l.* If, therefore, we admit an equal quantity of lines of road, to be undertaken and completed in the course of three years, would require an annual supply of 50,000*l.*; and supposing this to be raised in equal moieties by government and the land-owners, it would amount to 25,000*l.* each: but as it is not likely the whole could be brought so immediately into operation, it may, with more probability, be expected that six years would be taken up in executing all the lines, which would reduce the annual supply to 25,000*l.* or 12,500*l.* from each party. But this is exclusive of the four great bridges, the total expense of which is reckoned at 37,000*l.*; if they were undertaken by proper persons they might be executed in three years, which would require an annual supply of 12,333*l.* 6*s.* 8*d.*; and if government was to grant one-half,

It would be annually	-	£. 6,166	13	4
To which add the former		12,500	0	0

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£. 18,666 13 4

Speaking therefore generally, if government was disposed to encourage these plans of improvement, and would agree to advance 20,000*l.* in each of the first three years, and 12,000*l.* in each of the last three years, it would then remain with the land-owners of the districts of country through which the lines

lines were to be carried, to come forward with their surveys and estimates, and subscriptions to one-half the amount of the expense, proving at the same time, to the satisfaction of government, that the proposed lines would be of public as well as private utility. It would be necessary also to provide that the works should be substantially executed, and that the land-owners or others interested should always lay out a certain sum before government advanced an equal moiety.

Means should be provided for the maintenance of the roads and bridges after they have been completed; and for this purpose there should either be a fund reserved, or a small toll laid on, to go in aid of the statute labour of the country. A very important consideration also, is the erecting and maintaining proper inns upon the roads.

Several of the houses which were built by government upon the military roads, are striking instances of the necessity there is of giving the people who are to keep the inns something else to depend upon besides what arises from supplying travellers; there should be some land attached to the house, at a rent to be settled by reference. I am not prepared to say what the quantity should be, or of what particular description; it is at present sufficient to point out the principle.

Upon the whole, as far as regards the bridges and roads, I can have no hesitation in stating, that they are of the greatest moment for promoting the improvement of the country, and for perfecting the connections with the fishing lochs and the Hebrides; and I shall hereafter endeavour to explain my reason for thinking that some share of the emigrations is to be attributed to the want of proper communications.

#### *Naval Stations.*

Before entering upon the subject of the Caledonian canal it will be proper to observe, that I again examined the Bay of Cromarty, and have procured some more information respecting it and the Murray Frith.

Mr. Henderson, the resident custom-house officer, a man of respectability, has, at my desire, ascertained the depth of water in the wells, and also what quantity flows from a spring about half a mile from the town. From this letter it appears, that at the depth of twenty feet from the surface of the plain upon which the town stands, there are four feet of water in their wells; when at Cromarty I learned, that at high springs this water is brackish, but it is generally used for washing, and other domestic purposes. At the distance of about half a mile from the town there is a fine spring, which was discovered by the late Mr. Ross during his ineffectual attempt

to find coal: from this circumstance it is called the Coal Well. This spring produces, on an average, upwards of twenty hogheads of water in an hour; this water might be collected into a reservoir at a small expense; from this reservoir it might be brought to the shore, which is a short distance, in pipes, and by means of hose run into casks in a ship's boat: to protect the boats, a small pier might be formed at this place.

Captain Duff, of the royal navy, who is well acquainted with the country, and has navigated the Moray Frith in a frigate, has been so obliging as to resolve some queries which I took the liberty of transmitting to him through his brother-in-law colonel Dirom. Captain Duff confirms the general report of the excellence of the Bay of Cromarty, and the entrance to it; his opinion respecting the navigation of the Moray Frith is quite as favourable as I have been led to advance in my last report. From what he states it appears, that even with a contrary wind a ship of war could clear the headlands in forty-eight hours from the time of leaving the Bay of Cromarty. From this account, persons conversant in naval affairs will be able to judge how far this bay would be suitable for a squadron destined to watch the mouth of the Baltic and protect the coast. It would be singularly well situated for convoys to vessels coming from the westward through the Caledonian canal, and when returning with the same trade; after seeing the merchant ships pass Fort George, the ships of war would be close in with their own harbour.

As an aid to this station, the harbour of Aberdeen might be made to receive frigates. This harbour is not embayed, and frigates might sail from it at all times. In my last report I stated the expense which would attend improving this harbour, and I distinguished what extra expense would be required to render it capable of receiving frigates, which appeared to be 33,700*l*.

The magistrates of the town, by whose spirited exertions several valuable improvements have already been effected, have authorized me to say, that they are ready to co-operate with government as far as their circumstances will admit. If frigates were stationed at Aberdeen, and large ships at Cromarty, a naval protection would be immediately obtained in that quarter at a very moderate expense; and experience would point out what future improvements were necessary upon this coast.

From Aberdeen, Peterhead, and Fraserburgh, a communication might be kept up with Cromarty by means of signals or a telegraph, or by land expresses to Nairn, where  
the

the frith is only three leagues over; or a fast sailing vessel would soon run with an easterly wind from Frazerburgh to Cromarty.

If Cromarty Bay was made a roadstead, there would be wanted a storehouse and some other conveniencies, which may be on a small scale, until the merits of the place have been fully proved: allow an expence here of 5000l.

*The Caledonian Canal.*

I passed along the whole line of this canal, that is, from Inverness on the east, to Fort William on the west coast. I took much pains to examine into the nature of the navigation of the lochs, their soundings, and anchoring places. For this purpose I applied chiefly to Mr. Guynn, who commanded the Lochness galley, in government service, thirty-six years; from him I obtained a very full and satisfactory account of Lochness: he also took the soundings of Loch Oich and a part of Loch Lochy for me.

From him I learnt that Lochness is twenty-two miles in length, and from one to two and a half miles in breadth; that its extreme depth is 135 fathoms, and generally 15 to 20 very near the shores; that there are six anchoring places on the north, and four on the south side; and that at each end of the loch there is a good anchorage in from three to five, eight, and ten fathom water.

That a vessel passing from the west to the east end before a westerly wind, or in a contrary direction before an easterly wind, has nothing to do but run before the wind the whole way, which she would do in from three to five hours, and with contrary winds in moderate weather she would work it in from twenty-four to thirty-six hours.

That the easterly winds generally prevail from March till the end of September, when the westerly winds set in, and continue for the rest of the year.

The soundings he took in Loch Lochy are from seven to sixty-eight fathoms, with bold shores and good anchorage.

In Loch Quoich, which is a short loch on the summit level, there are some shallow parts, but they may be made sufficiently deep.

I next proceeded to examine the supplies of water which are on the summit level, and for this purpose I passed up the valley of Glengarey to Loch Hourn at the back of Skye. In this valley I found Loch Garry, which appears to be upwards of five miles in length, and from half a mile to one mile in breadth; towards the head of the valley is Loch Quoich, which appears to be upwards of eight miles in length, and

from one to two miles in breadth: these natural reservoirs, placed in an extensive valley and a rainy country, form an abundant provision for every purpose to which water can be applied in the course of the canal.

Having ascertained these points, I passed by a very rocky and precipitous tract down to the head of Loch Hourn; from Loch Hourn I travelled by a tract scarcely less rugged to the top of Glen Elg, and over the steep mountain of Raatachan to the top of Loch Duich; from thence I travelled along the vestiges of a military road, up Glen Shiel, down a part of Glen Morrison, and over a rugged mountain to Fort Augustus. In Glen Morrison and Glen Garry it is possible to make roads, if judiciously laid out, upon an easy ascent; but the idea of water conveyance through them between the Caledonian canal and the fishing lochs is altogether unadvisable.

My next object was to examine the country which lies between the top of Loch Eil and Loch Shiel, in order to find whether a water conveyance could be made at a moderate expense from Loch Eil through Loch Shiel, and so into the fishing grounds to the south of Skye, without passing down the Linnhe Loch, up the Sound of Mull, and round the Point of Ardnamurchan. I carried a level across the neck of land which separates Loch Eil from Loch Shiel: the distance is about three miles. I found the summit of the land forty-three feet above high water in Loch Eil, and thirty-five feet five inches above the level of the fresh water in Loch Shiel. In order to form a canal, as there is no water to be got on this summit, the ground must be cut down to twelve feet below the level of the water in Loch Shiel, which would make forty-seven feet five inches of cutting, and this depth of cutting would be continued for nearly a mile: I also suspect that in this distance rock would be met with. I am therefore sorry to say, I cannot advise the work being undertaken before the nature of the ground has been fully proved, and the whole of Loch Shiel has been examined with great care: and as this passage cannot be useful unless the Caledonian canal be made and navigated from the east, if that event should take place, there will be plenty of time to re-examine this point with care; at present it ought, in my opinion, to be postponed.

Having investigated all the points which fell under my observation as a civil engineer, I became extremely desirous of having the opinion of experienced and well-informed mercantile and sea-faring people with regard to the present navigation by the Pentland Frith and the Orkneys, and the proposed inland navigation by the Caledonian canal. With  
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the view of procuring the best possible information on this subject, I applied to Leith, Aberdeen, and Peterhead, on the east coast; and to Greenock, Dublin, Liverpool, and Bristol, on the west.

In the appendix to this report I have inserted the queries and answers; by which it may be seen that there is only one opinion as to the dangers and inconveniencies of the present navigation, and the advantages which may be expected from the proposed inland navigation, if united with a naval station in the Moray Frith, or on the adjacent coast of Scotland.

This sanction of experienced people, who are all deeply interested in commercial concerns, will, I trust, satisfy your lordships, that it has not been upon unsubstantial grounds that I have ventured to recommend this great national object.

My estimate of the expense of forming this navigation is nearly 350,000*l.*, and the time required to complete it would probably be about seven years: this division would require an annual supply of 50,000*l.*

Upwards of thirty vessels have been wrecked on the coast of Caithness in the memory of Alexander Miller, of Staxigo.

[To be continued.]

LVI. *On Winds.* By RICHARD KIRWAN, *Esq. LL.D. F.R.S. and P.R.I.A.\**

*Of the Origin of the general Trade Winds.*

THOUGH the origin of the general trade winds appears to me to have been fully established by Dr. Halley, yet it seems he has explained himself too briefly, since his explanation has been misunderstood by many, and was thought obscure even by d'Alembert †.

To understand it more perfectly, let us suppose the sun for the first time in the meridian, and to communicate its heat every instant fifteen degrees all around. If it were to remain in this situation the surrounding air could have no other motion but upwards, for the lateral dilatations being equal, would necessarily check each other, but in the second, and all the succeeding instants, the sun moves westwards; therefore, of the originally equidistant eastern and western points,

\* From his paper entitled "Of the Variations of the Atmosphere 1801."

† Sur la Cause des Vents. v.

the western, to which the sun approaches nearer, is more heated than the eastern, from which the sun recedes; therefore in this, and all the succeeding instants, the eastern, being more cooled, will press on the western, and thus an eastern wind will be established.

It is true, that, in the northern hemisphere, the northern air also presses upon the more heated spaces; but as this also follows the sun's path to the westward, it becomes also easterly, preserving only a few points of its primitive direction. D'Alembert adds also the solar attraction, which, according to him, elevates the air in the points over which the sun is vertical, and consequently produces a dilatation advancing from east to west. But M. De la Place, not denying this cause, considers it too weak to produce singly any considerable effect\*.

About the year 1735 Mr. Hadley published a very different account of the origin of the trade winds (*Phil. Trans. Abrid.* viii. p. 500); which, however, has been rejected by the most distinguished astronomers that have since attended to this object, as d'Alembert, *sur la Cause des Vents*, art. 376 and 385; *Gentil Voy.*; *Bergman Erde Beschreib.* ii. p. 91.

According to Mr. Hadley, the air, being rarefied towards the equator, is consequently invaded in the northern hemisphere by the northern, and in the southern hemisphere by the southern colder air.

But as the parallels of latitude enlarge as they approach the equator, and as the equatorial space is nearly in the proportion of 1000 to 917, the difference of their circumference is nearly 2083 miles; consequently, the surface of the globe at the equator moves so much faster than under the tropics; and hence the northern or southern air, moving from the tropics towards the equator, must possess less velocity than the parts it arrives at, and consequently appear to move in a direction contrary to that of the earth's motion; which being from west to east, the air arriving sooner at the western parts, will appear to move from east to west; and this relative motion being combined with that towards the equator, a north-east wind will be produced on the north side, and a south-east wind on the south side of the equator. These as they approach the equator should become stronger and more easterly, and appear due east in the equator itself, by reason of the concurrence of both currents from the north and from the south. There the velocity of each should be at the rate of 2083 miles in the space of one natural day, or above 1.33

\* *Mém. Paris* 1776.

miles per minute, if it had not been that before the air at the tropics could arrive at the equator, it must have gained some motion eastwards from the surface of the earth or sea, whereby the relative motion is diminished to the degree that actually exists in it.

This theory appears to me rather ingenious than solid, for the following reasons :

1. The trade winds are commonly gentle, moving only at the rate of eight miles an hour ; therefore they have sufficient time to gain or participate of the motion of the earth ; therefore their contrary course must arise from an absolute cause, and cannot be deemed merely relative.

2. Because the north-east wind scarce ever approaches nearer than eight or ten degrees to the equator, and there dies away ; whereas it ought there, according to this theory, to be strongest. And, on the contrary, the south-east passes the equator several degrees, even when the sun is in the south tropic. A fact which, as Gentil remarks, is absolutely irreconcilable with this theory. (*Gentil Voy. i. p. 638 ; Ibid. v. p. 116.*)

3. Because, if the constant easterly wind was in the northern hemisphere supplied solely from the north, and in the southern hemisphere solely from the south, we should in the former have a constant north wind at least at 35 or 40 degrees from the equator, or at least from some northern point, and in the latter a constant south wind, or at least from some southern point ; whereas, on the contrary, a south wind often prevails in those latitudes on the north side of the equator, and a north wind on the south side. Thus La Perouse met an E. S. E. in north latitude 32°, and a due east in latitude 31° ; and a S. S. E. in latitude 14°, and a due east in latitude 16° ; and a due north in latitude 20°, (where then was the relative motion ?) and a due south in latitude 33°. (See his Journal in *La Peyr. Voy. iii.*) He also met with a due north in latitude 27° and 42° south, and a N. N. E. in latitude 25° south. So captain Cook met a S. S. E. wind in latitude 30° north, and also in latitudes 40° and 41°, and a due south wind in latitude 38° and 20° ; and in the southern hemisphere a due north in latitude 3°, 4°, and 44°. I might produce other instances from sea journals, and particularly from that, most ample and instructive, kept by major Dalrymple during a voyage to the East Indies (*Phil. Trans. 1778*) ; but I think the alleged sufficiently prove that the general east wind is not supplied solely from the north or south in the different hemispheres respectively.

4. Because, during our six summer months, when the sun is in or approaches to the northern tropic, the easterly trade  
wind

wind partakes less of the northerly, than when the sun is in or approaches to the southern tropic, (Phil. Trans. Abrid. ii. p. 134; and Schued. Abhandl. 1762, p. 175 :) which is directly contrary to Hadley's system; for when the sun is in the southern tropic, the north wind must traverse more of that space in which the earth's motion eastward is strongest, and therefore should participate more of that motion, as Hadley himself states: though still partaking of it in a smaller degree than that which the globe itself possesses, it should appear to move westwards; yet it should proportionably retain less of its original direction from north to south than when it had traversed a space more distant from the equator, whereas the fact is that it retains more, and often passes into the southern hemisphere into the  $13^{\circ}$  south latitude without having any eastern direction, (Marchand iii. p. 551;) and an analogous fact is observed with respect to the south-east wind when the sun is in the northern tropic. Hence it is evident, that it is from the approach of the sun, and not from the latitude traversed, that the eastern direction is derived; nay, the wind is often more easterly than northerly between latitude  $23^{\circ}$  and  $28^{\circ}$ : (Foster's Observations, p. 126.) He even observed that the trade winds extended far beyond the tropics when the sun is in the same hemisphere, which shows it is the sun that causes them.

*Eddy* is a term introduced on this subject, which explains nothing when its cause is not assigned and proved; the trade winds are often interrupted by the approach of land; but the interruption, as Foster mentions, extends only to a few miles. Ibid. 127.

The *monsoons* or periodical trade winds depending on local circumstances, sufficiently explained by Dr. Halley, I shall here pass over; though certainly much may be added from observations made by subsequent navigators and travellers. I shall therefore confine myself to the *variable* winds, a subject much more obscure.

### Of Variable Winds.

With respect to winds we must lay down one general and fundamental principle, which is, that they always originate at the extremity of that point towards which they proceed. Thus the easterly trade wind begins at the point nearest the sun, which it follows, and is perpetually renovated and supplied from parts still more easterly. Thus in the year 1709 a north wind was sooner perceived in England than at Dantzic (Phil. Trans. Abrid. iv. part ii. p. 115. And Wargentín notes, that when the wind changes to the west, this change takes place at Moscow before it happens at Abo, which is several

several degrees west of it; and sooner in Finland than in Sweden. (Schwd. Abhandl. 1762, p. 195.) And Dr. Franklin, in his xxxvith Letter, p. 389, thinks that the north-east storms in North America begin first, in point of time, in the south-west parts; that is to say, sooner in Georgia than in Carolina, and sooner in Carolina than in Virginia, &c. He found that a north-east storm began at Philadelphia at seven o'clock, but did not extend to Boston (about forty miles to the north-east) until eleven o'clock. The reason of which he well explains, as the current must begin in the places nearest to that in which the rarefaction arises, towards which the current is directed.

### *Of Westerly Winds.*

That eminent and laborious meteorologist C. La Cotte, infers from numerous observations of many years, that between latitude  $47^{\circ}$  and  $60^{\circ}$  on the western side of our hemisphere, the west wind, with some participation of the north or south, is that which obtains oftenest. (Roz. Jour. xxxix. p. 267.) Leche obtained the same result at Abo, latitude  $60^{\circ}$ ; from twelve years observations; Muschenbroeck, in Utrecht; Mr. Dalton, in Westmorland, latitude  $54^{\circ}$ , (see his Meteorological Essays, p. 48 and 88,) from five years observations.

This wind in our continent originates in the Pacific Ocean between the above-mentioned parallels, at least in winter; the air incumbent on that ocean is then much warmer than that of Siberia and Chinese Tartary that lie west of it; this therefore presses upon and flows into the supra-marine, and is immediately succeeded by air still further westwards, and thus a current is gradually established extending to the Atlantic, which, though in winter, being much warmer than the air of the islands and continent on which it flows, is forced into the current, both by the rupture of the equilibrium to the eastwards, and by the pressure of the much colder air of the continent of North America.

### *Of Easterly Winds.*

During the winter months there seems to be a frequent struggle and contest betwixt the air incumbent over the Asiatic continent and that incumbent on the North American lying betwixt the above-mentioned parallels and bordering on the Pacific Ocean, which of them shall rule over it.

The mass of the American air being less considerable, and its efforts divided between the Pacific and the Atlantic, is generally obliged to yield to its antagonist; though sometimes the Asiatic being warmed, either by a diffusion of the superior

perior current or by southerly winds, the colder American becomes more forcible. In summer this must happen frequently, the E. N. E. ofteneft prevailing: upon the whole, however, Leche remarked that the east and E. S. E. were nearly the most uncommon; as did La Cotte in the climate of Paris. (*Meteorolog.* p. 305.)

With us this wind is most frequent in the months of April and May; and I have observed in Cook's Journal, tables 9th, 10th, and 11th, that it prevails also in the same months in the Pacific, therefore the colder continental air then pours in upon us.

La Cotte also observes, that in the western tracts of Europe, in latitudes below  $48^{\circ}$ , this wind occurs ofteneft during the winter months\*; for the superior heat of the Atlantic in the low latitudes determines the colder air incumbent on Hungary and European Turkey to flow in upon it.

### *Of Southerly Winds.*

A few years ago, no problem in meteorology appeared to me more difficult than to assign a cause for the frequent prevalence of a south wind even in winter, it being contrary to the laws of nature that warm air should rush upon colder; yet I since discovered that the conjectural solution I then offered is grounded on a real fact.

In the eastern parts of our hemisphere, from longitude  $72^{\circ}$  to  $160^{\circ}$ , that is, from the coast of Malabar to the Moluccas, it blows from the north-east constantly from October until April. Now this northern blast must be supplied and recruited from countries still further north until we arrive at the pole, and the polar air must consequently be supplied by that which lies south of it, and thus a southern current is established on the western side of our hemisphere.

Instances to support or contradict this theory do not often occur; yet I have found some that appear to me decisive, independently of the general reason alleged. Thus I find in the ninth table of the third volume of Cook's Voyages, that in north latitude  $59^{\circ}$ , and east longitude  $207^{\circ}$ , on the 25th of May 1778, a strong north-west wind prevailed; and on the 29th day of the same month and year, an equally strong south-west wind prevailed at Petersburg, latitude  $60^{\circ}$  and longitude  $30^{\circ}$  east. Now the places of observation were 177 degrees distant, one on the eastern and the other on the western side of our hemisphere, (which, at this proximity to the pole, argues not a superior distance to that I have men-

\* *Mem. Meteorolog.* ii. p. 189, &c.

tioned;) and four days is as short a time as can be allowed to the south-west to supply the more eastern north-west. (Mem. Petersburg 1778, p. 92.) So also in the same journal I find, that from the 4th to the 30th of May a north wind prevailed in the eastern part of our hemisphere from latitude  $58^{\circ}$  to  $61^{\circ}$ , except seventeen days of variable winds; but in London it blew from the south-west during the first fifteen days of June, thus replacing the northern air. And to replace the constant north-east wind on the Indian peninsula to the Moluccas, there is a constant draught from the south in the western parts of our hemisphere; accordingly Leske observed, that on an average of twelve years it blew 126 days each year, from October until May, from some south point, namely, 86 days from the south or south-west, and 40 from the south-east, at Abo, latitude  $60^{\circ}$ .

It is true, that he found it to take place very frequently also in summer; but this is occasioned by the great heat that then prevails in the northern tracts of Lapland.

And, upon the whole, more of the south air is drawn off in winter than in summer; for its flow is gentle in summer, but often stormy in winter. See Leske's 9th, 11th, and 12th tables. If all other meteorological tables of a series of years had been arranged with equal sagacity and precision as those of Leche and Dr. Horsley, a vast fund of information might be extracted from them.

At Petersburg, during the year 1793, Euler junior found a south or south-west wind prevailed 79 days, 52 from October to the end of March, and only 27 in the summer months: it was stormy in November, December, and January. I have not noted the south-east.

Mr. Stritter also found the south wind to predominate at Moscow during the six winter months of that year, (N. Acta Petrop. xi. p. 569;) so that the frequency of this wind in high latitudes is certain.

### *Of Northerly Winds.*

In the western parts of our continent and hemisphere these are of all others the least frequent in latitudes above  $48^{\circ}$ : See La Cotte's and Leche's tables. The cause of this unfrequency appears from what has been said of south winds.

But in latitudes below  $48^{\circ}$  they occur oftener, and ofteneft in those that are still lower, as La Cotte remarks. An admirable instance of Divine Providence, that the warmest winds should prevail ofteneft in winter in the coldest regions, and cold winds in the warmest!

But it may be asked, why a south wind should not prevail

in the eastern parts of our hemisphere to supply the constant north-east wind that prevails in the low latitudes of the western side? The reason is, that on the western side the north-east winds of low latitudes are easily supplied by the contiguous Atlantic, which is open up to the North Pole; and, as here, the upper current lets and ceases, there can be no deficiency of air.

*Of opposite concomitant Winds.*

It has often been observed \*, but of late, since the invention of balloons, evidently proved, that currents of air from different and even opposite points of the horizon, prevail at different heights in the atmosphere over the same tracts of land or water. This was originally inferred from the different courses of the higher and lower clouds; but as such observations were often liable to optical deceptions, better proofs were wanting.

Mont Louis is within thirty miles of Perpignan, but about 5000 feet higher. Now in March 1780, north and north-east winds prevailed at Perpignan and a westerly wind at St. Louis. In August a north wind prevailed at Perpignan and an east at Mont Louis. *Mém. de la Société de Médecine de Paris 1780.* Derham suspected †, and Gentil has since shown, that changes of seasons constantly begin in the upper atmosphere; while a strong wind blows from one point below, a wind from an opposite point reigns above, but more gentle, until at last (in about three weeks) it is propagated downwards. (*Voy. ii. p. 23, 24, in 8vo.*) The lower atmosphere, he says, extends to the height of 2880 feet. (*Vol. iv. p. 48.*) At the commencement of winter, when the sun approaches the south tropic, and the north air begins to flow in and follow it, it must meet with more resistance from the lower denser air, as its impetuous course in an opposite direction is more slowly altered (this respects the monsoons) than in the rarer superior strata; and the same effect, but in a different direction, takes place when the sun approaches to the northern tropic.

It has been said by many, that winds in the superior regions of the atmosphere are much more violent and impetuous than in the lower. (*Saussure Hygrom. p. 300: Ulloa's Voy. ii. p. 81: Muschenbr. § 2612: Bergm. Erde kugel. ii. p. 99: De Luc, &c.*) But the contrary has also been observed by Gentil, above quoted, and Morveau. (*Aëroft. de Dijon.*)

\* Ulloa's Voy. ii. p. 62. English.

† Phil. Trans. Abridg. iv. part ii. 125.

*Of the Succession of Winds.*

Well established general laws on this head would be extremely useful, as we might then foresee what wind might next be expected. Besides the *general* succession in an open country, it is probable there is a *local*, confined to certain situations.

Gentil remarks, that in the southern latitudes of our hemisphere, a north-east is succeeded by an east, south-east, and south. According to La Cotte, the order of succession in the middle latitudes is south-west, north, west, north-east, south, north-west, east, south-east. (Roz. Journ. xxxix. p. 267.)

*Of the Scirocco.*

This is a south or south-east wind, known in the southern parts of Italy, Sicily, and Malta, distinguished by peculiar debilitating effects, well described by Brydone, and by Dolomieu in his Treatise on the Temperature of Malta. The latter has shown that its malignity results from the constitution of the air it conveys, and not merely from its temperature, which is variable, from  $55^{\circ}$  to  $80^{\circ}$ . It contains a much smaller proportion of oxygen than air usually does. The constitution of the African wind, called *harmattan*, is as yet unknown; it is, at least on land, loaded with some unknown undissolved vapour, and is much hotter and drier than the *scirocco*, but not debilitating, and even wholesome for animals; for though it parches their skin, it destroys infection and cures several disorders. (See Phil. Trans. 1781, p. 46, &c.) Its direction is also westwards.

LVII. Report presented to the Class of the Exact Sciences of the Academy of Turin, January 12, 1803, on the Action of Galvanism, and the Application of this Fluid and of Electricity to Medicine. By A. M. VASSALI-LEANDI\*.

THE Galvanic experiments made on the 10th and 14th of August last, in the presence of a great many spectators, by Giulio, Rossi, and myself, on the head and trunk of three decapitated criminals, an account of which has been published, gave rise to several questions in regard to this agent, and by analogy respecting electricity. These two fluids, and the uses to which they may be applied, have become a common subject of conversation among well-informed per-

\* From the *Journal de Physique*, Germinal, an. 11.

sons, and, as is always the case with new discoveries, their uses are exaggerated by some, and despised by others. This diversity of opinion induced our associate Charles Bosse to propose to me two questions, which I endeavoured to solve in the next letter I wrote to him about the end of September. I could easily increase the number of cures obtained by means of Galvanism and electricity, as well as of the misfortunes occasioned by the action of these fluids and of the instances where I observed a constant relation between the moral part; that is to say, the strength of mind, the courage of the victims to justice, and the effects of the Galvanic fluid on their bodies: but these observations belong to the general report of the experiments which we made since the 14th of August. I shall therefore only present to you the letter which I wrote at that period.

“ You asked me in one of our late academic sittings, why, after so many experiments, made by the first philosophers of the last century, on the electric fluid, such a variety of opinions is entertained in regard to its medical action in the human body; and whether Galvanism seems already to promise results more useful to the healing art. I shall here give you my opinion on these questions, or rather submit to you the inductions which different experiments made by myself, or at which I assisted, gave me reason to deduce with more certainty than I durst venture to hope when I began to pay a serious attention to this object.

“ I consider Galvanism as a modification of electricity—a modification which renders this fluid more active; as the small flame separated by the blow-pipe is far more ardent than the large one from which it is taken. I have read to the class several experiments which seem to support this comparison between electricity and Galvanism. Animals which were only stunned by the strongest sparks from a magic picture, were killed in less than three minutes by a very weak degree of Galvanism.

“ The fluid of a pile composed of 25 plates of silver and zinc of the size of a crown-piece, intermixed with pieces of paste-board moistened in water saturated with muriate of ammonia, oxidates the metals in decomposing the water, while it is scarcely sensible in the fingers, and gives only small sparks. The brilliant electric sparks which excite in our bodies a strong sensation of pricking, neither oxidate the metals nor decompose the water if they do not communicate a shock. Having made the Galvanic current to pass through the body of a frog, its fluids were decomposed, and I saw it swell up so much that it could no longer plunge into the water though possessed

possessed of great vitality, which I never observed to be the case in frogs when tormented by strong electric sparks. All these facts, to which many more of a similar kind might be added, confirm the great activity of Galvanism in comparison of electricity. Hence it results, that the fluid of the Voltaic pile may be very useful in cases in which common electricity would not have sufficient activity. You are acquainted with some of the experiments which I made in conjunction with my colleagues Giulio and Roffi: we made others, still more interesting, which have determined physiological facts, before doubtful for want of being verified. We then tried an application of it in several diseases with the greatest success. Three of the cases are as follow:

“A lady about thirty years of age, after severe pains in the head, lost the sight of the right eye. C. Roffi being consulted in regard to this malady, after a close examination of the eye, which appeared to be as sound and to look as well as the left, concluded that it must arise from a palsy of the optic nerve, or what is called a *gutta serena*, which suffered the patient to see only, as it were, through a thick mist; which increased her misfortune, since it deranged the sight of the other eye, so that she was always afraid of falling, not being able to distinguish well with the right eye the objects which she handled. Roffi being sick, sent her to me, that I might make an application of Galvanism. I formed a pile of thirty pairs of plates like those already mentioned, and employing gold wire as conductors, I caused the Galvanic current to enter near the exterior angle of the eye, and to issue sometimes at the eyebrow, sometimes by the ophthalmic ramus which passes through the orbital foramen, and sometimes near the interior angle of the same eye. The operation was very painful; it caused abundance of tears to flow; but, after successive Galvanic shocks for half an hour, the eye began to see a little better. That I might not fatigue my patient too much, and that nature might have time to act, the operation was suspended till the evening, when it was repeated for half an hour. The next day the eye began to distinguish the figure of bodies. Having repeated the operation for three days following, the lady was not only able to distinguish the figure of bodies, and people's features, but also the pupils of their eyes. Before this operation, in consequence of a consultation with Dr. John Baptist Anforini, first physician of the Hospital de la Charité, I had Galvanized a young woman, twenty-seven years of age, of a melancholy temperament, who, after some slight attacks, had a hemiplegia of the right side, which affected in particular the arm,

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the cheek, and the eye. The other symptoms were removed by bleeding and the use of the remedies usual in such cases; but the eye always remained fixed, with pains in the muscles. The application of Galvanism for ten minutes was sufficient to excite abundant tears and a discharge of a watery fluid from the nose on the side which had always been shut since the attack of the disorder, and greatly alleviated the pains of the muscles. She could even turn her eye to both sides; but she found great difficulty in raising or lowering it, with a sensation of heaviness all around the eye. This operation being repeated, after the interval of a day the eye acquired its former freedom of motion, and the patient was freed from every sensation of uneasiness.

“ These two operations were performed in the presence of several persons, and executed almost entirely by C. Hyacinth Carena, lecturer on natural philosophy in the National College of Turin.

“ The advantages of Galvanism will appear to you still more decisive, by the cure of a person labouring under hydrophobia, performed lately by C. Roffi, who will give a full and complete account of it in a memoir on which he is now employed.

“ A man bit in the finger by a mad dog, came to consult him, in consequence of a pain which he felt in the arm, the back, and particularly the finger, which had been bit more than a month. A caustic applied to the finger removed the pain; but a few days after it returned, accompanied with symptoms of hydrophobia. The patient could no longer look at water without horror; an inflammation in the throat prevented him from swallowing even chewed bread, and he experienced a strong propensity to bite those around him.

“ In this state he was brought to C. Roffi, who observing that he could not bear the sight of water, nor that even of shining bodies, provided in another room a pile consisting of 50 pairs of plates of silver and zinc intermixed with 50 pieces of pasteboard moistened with a solution of muriate of ammonia. He employed slips of brown paper moistened as a conductor, on which the naked feet of the patient were placed, and at the moment when he opened his mouth to bite, one end of the arc was thrust into it, while the other communicated with the pile. The patient suffered a great deal from this operation, which after several shocks weakened him so much that he could no longer support it. Being stretched out on the floor, he was then Galvanized with ease: the operation made the sweat run from him in drops. After some time Roffi caused the patient to be conveyed home, and gave

gave orders that he should be brought back next day, that the operation might be repeated. It was two o'clock in the afternoon when the patient was Galvanized, and at six next morning he came to Rossi himself to tell him that he was completely cured, as he experienced no pain or difficulty of swallowing, and was entirely freed from his aversion to water and to liquids: no persuasion, however, could induce him to submit to a new operation.

“But a few days after, some slight pains having given him reason to apprehend a new attack of hydrophobia, he returned to Rossi, who by repeating the operation made all the symptoms disappear. This cure was also effected in the presence of several persons. The patient was endowed with so great sensibility, that for more than a month after he felt in the shoulders a sensation of the Galvanic shocks, which I felt only as far as the articulation of the finger, though I am not one of the least sensible. You see by these trials what are the advantages which may be hoped for from Galvanism. I entertain no doubt that a man so active may preserve from the grave many individuals, by Galvanizing them at the moment when the play of the vital organs is suspended by an accidental cause.

“This will become more evident by an explanation of the medical action of electricity on the human body.

“Several celebrated writers have classed electricity among those remedies which are most certain and most active; others have shown the inutility, and even danger, of this fluid considered as a remedy; and both seem to be supported by well attested facts.

“Nothing, however, can be more easily explained, if we reflect, that most of those who have applied electricity to medicine have been guided by quackery, without consulting the nature of the disease, or of the agent which they employed. For this reason I stated in the memoir which is about to appear in the Transactions of the Academy, that the greatest circumspection should be observed in the use of Galvanism, which, like electricity, may be attended with bad consequences; and even advanced, that the latter, though a very good remedy of itself, has done more hurt than good by improper application.

“I shall not here speak of the chemical properties which during the enthusiasm of novelty has been ascribed to electricity, such as that of transfusing into the human body the action of remedies enclosed in glass tubes, by rubbing it with them.

“It is well known that it is the fate of new discoveries to be

exaggerated, so that their adversaries easily find reasons to oppose them; but after some time things come to their proper level, established on a more complete knowledge of the agent; and those well acquainted with the properties of electricity are able to distinguish the cases in which it may be administered with advantage, from those in which it would be only prejudicial. Of ten patients affected with the same disease, which undergo the same electric treatment, five may be entirely cured, and the other five be exceedingly ill.

“Those who are cured extol electricity as the best of remedies; those who have suffered, say that it only aggravates the evil. Both speak from correct experience, and at the same time right and wrong, for attempting too much to generalize; that is to say, because they do not distinguish the cause of the disease which requires or opposes the application of electricity. Thus the same pain, the sciatica\* for example, may be occasioned either by a stagnation of the fluids, by their too great abundance, or by the want of reaction in the solids: it may arise also from organic defects; an alteration of the fluids; a poisonous, or, as it is called, *acrid* principle; or from a peculiar virus in the fluids.

“The five patients who labour under a stagnation of the fluids, receive the greatest relief from electricity, which puts them in motion; the other five, tormented by sciatica occasioned by vitiated humours, will grow worse under electric treatment, which will increase the acridity of the humours, carrying off a part of the water which kept the poison diluted. This theory of the effects of electricity in the human body is founded on the nature of this fluid, and on its properties established by numerous experiments. The electric fluid tends always to put itself into a state of equilibrium, and this tendency is so strong, that it penetrates to a certain distance in the air, and extends along idio-electric bodies. It is this tendency which causes water when electrified to flow from capillary syphons, whereas a very few drops only flow without this electrization. It is by the same tendency that the electric spark, when it passes from one conducting body to another by non-conducting substances, carries with it in its passage conducting particles which serve it as a vehicle, if the solidity of the bodies does not oppose a very strong resistance. This property, proved by the common effects of thunder, and by several experiments, serves to account for the

\* Dr. Balbis observed to me, that sciatica of every kind may be accounted for without recurring to the hypothetic alteration of the fluids. I replied, that I wished only to compare my theory with the principal theories of sciatica, without concerning myself with their probability.

great evaporation of electrified liquids, and for the greater perspiration of animals and vegetables which have been electrified. It is then evident, that whenever a stagnation of the fluids takes place, if other symptoms do not oppose it, electricity will be a good remedy: on the other hand, if the disease arises from vitiated fluids, or a virus diluted in them, electricity in this case, either by the evaporation of the diluting fluid, or by the greater alteration it may produce, will be hurtful. From what has been said, it is evident that electricity and Galvanism ought to be employed with the greatest circumspection, and that the nature of the disease ought to be compared with the nature and properties of these fluids, to ascertain whether the application of them is proper or not. It is to be observed also, that this remedy, in consequence of its activity, may be dangerous, like all other remedies, however good, if abused.

“I could adduce several instances of misfortunes occasioned by the abuse of electrization, even in cases in which a little time before it had been indicated; but I shall mention only one fact in regard to Galvanism:—A young woman was cured by Galvanism of pains which she experienced in the muscles of the face. After the cure, having continued to Galvanize herself, she did hurt, which increased with the application of the Galvanism, and did not cease till she abandoned herself entirely to the powers of nature, assisted by good nourishment. The patient then, who is incapable of forming a proper opinion respecting the state of his health, should consult a good physician, one of those who do not despise natural philosophy and the new discoveries, in order that he may never suffer by the application of electricity or of Galvanism, which, as Boerhaave says of another very active remedy, *Mira præstat in multis incurabilibus; at prudenter à prudenti medico abstinere si methodum nescis* \*.

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LVIII. *Account of some Remains of a Species of gigantic Oxen found in America and other Parts of the World.* By Mr. REMBRANDT PEALE †.

AMONG the remains of gigantic and unknown animals found in America, we have lately discovered one of the ox or buffalo kind, which was taken from the bed of a creek falling into the Ohio, 12 or 14 miles north of Bigbone-lick, and presented by Samuel Brown, of Kentucky, to the Philoso-

\* Elem. Chemiæ, pars iii. processus 193.

† Communicated by the Author.

phical Society at Philadelphia. By permission of the society I made a plaster cast of this extraordinary bone, which I have now with me in London, and of which I send you an accurate drawing, representing the back part of the head, with the condyles of the neck, and the pith or internal part of the left horn at the base. (See Plate VI.) The right horn is broken off, and all the fore part of the head; but from the fragment remaining it is a reasonable conjecture that the buffalo to which it belonged was about 10 or 11 feet high. The horn at the base measures 21 inches in circumference, and tapers very gently towards the extremity, where it is broken off; so that the horn could not have been less than six feet in length. From the middle suture on the head to the base of the horn measures  $7\frac{1}{2}$  inches; consequently the two horns were 15 inches distant; which must have been increased when they were partly covered with flesh, skin, and hair.

It is very extraordinary that bones of this kind have been occasionally found in Siberia, in Italy, Germany, and other parts of Europe, though not quite so large as this American bone; which circumstance, strengthened with others of a similar nature, must prove, either that these great animals have inhabited those various countries, or that their remains have been forcibly scattered by the action of water.

Buffon informs us\*, that in the parish of Haux, a mile and a half from Langoiran, in the splitting of a great rock, some large bones, mostly petrified, were discovered; probably of the ox kind, but of a very great magnitude. He likewise mentions†, that in 1772 there was found near Rome an ox's head in a state of petrification. The length of the forehead between the two horns was 2 feet 3 inches; the distance between the orbits of the eyes 14 inches; that from the upper portion of the forehead to the orbit of the eyes, 1 foot 6 inches; the circumference of one horn, 18 inches; the length of the same following the curve, 4 feet. "This instance is sufficient to prove," says Buffon, "that there have been prodigious giants among this species of animals; but it is further confirmed by other facts." He then enumerates several bones of the same kind in the museum at Paris similar to some which I have remarked in the British museum.

In the Philosophical Transactions‡ there is an engraving and an account of a bone of this kind, found near the city of

\* Quarto Supplement, vol. v. p. 486.

† Page 543.

‡ Vol. xxxvii. p. 427.

Dirschaw, of very large dimensions, and having the same characters which distinguish the one found on the Ohio.

Until the discovery of this bone in America, the tradition of the Indians concerning the great buffalo has been considered as entitled to very little attention. Many interpreted it as having entire reference to the mammoth, whose pre-eminent size was obvious, and whose carnivorous teeth were well calculated to excite terror; but I have now no hesitation in believing that the tradition, which, with such little variation, prevails through all North America, mentioning the antient existence of a great buffalo, is a tradition really handed down to them from their forefathers, but, like all other traditions, clouded with fable: yet it is not improbable, since we find the remains of the mammoth and the great buffalo in the same country, that the distinct ideas of each have been in time confounded, the terrible *power* of the one with the *name* of the other.

It has been too much the custom, whenever any large bones have been found in Europe, to call them all elephants' bones; and in America, to think them all belonging to the mammoth: but from the progress now made in this inquiry there is reason to hope for additional light on this interesting subject, whether it be considered as a foundation to theological faith, or regarded as a conspicuous monument in the history of the world.

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LIX. *Description of the Nymphæa Cærulea.* By JULIUS CÆSAR SAVIGNY, of the Institute of Egypt\*.

IT is well known that the lotus of the antient Egyptians was one of the most celebrated plants of antiquity. Rising every year with the waters of a river which overflowed its banks only to fecundate the earth; springing up amidst plains formerly desert, which it embellished with its beautiful flowers; and cultivated to serve as food to the least sensual, but the most numerous class of the inhabitants; it was judged worthy of homage by one of the first people in the world, who considered it as the happy sign of abundance, and as a sacred pledge of the favour of the gods.

It is to the genus of the *nymphæa* that the modern botanists have referred the lotus, which has been described by most of the antient historians, and which is engraven on all the antique monuments of Egypt. Two species of this genus,

\* From *Annales du Muséum National*, No. 5.

one with white and the other with azure flowers, still ornament, during the time of the inundation, the surface of the canals and rivers, and in general of all the fields of Lower Egypt, which are covered by the waters of the river. Plants so similar were no doubt beheld by the Egyptians with equal veneration; and though Linnæus, who was well acquainted with the former, gave it the name of *lotus*, it is to be presumed that the second procured the same advantages and served for the same mysteries. Its flowers have more splendour and a sweeter smell, and the azure colour they exhibited might become, among a people so religious, the emblem of the residence of the deity on the waters.

The *nymphæa lotus* is very well described, and a good figure is given of it, in several Systems of Botany; but the case is not the same with those the flowers of which are azure-coloured. It was scarcely observed by a few travellers, and if mentioned, it was only in a vague and incorrect manner. Forskal, who travelled through Egypt in 1761, and who gave a Flora of that country, did not observe it. His silence in this respect is the more astonishing, as the plant in question and the *nymphæa lotus* grow together and usually intermix their leaves. It is not even indicated in the last edition of the *Systema Naturæ*, published by Gmelin. This author, however, collected with great attention all the scattered species to be found in the different works which have hitherto appeared\*.

It is therefore indispensably necessary that a name and specific character should be assigned to this nymphæa; it is also necessary that a correct description should be given of it: this, in some measure, will be enriching botany with a new plant; and this, indeed, is the object which I propose in the present memoir.

But as this species has a great resemblance to the *nymphæa lotus*, and as it is possible that they may have often been confounded, I have thought it will be of advantage to give a comparative description of both. By these means the reader will be better enabled to comprehend the characters by which they are distinguished, and which render it necessary to form of them two separate species.

The root of the *nymphæa lotus*, and that of the other species, which from the colour of its flowers I have called the azure nenuphar (*nymphæa cærulea*), consist of long, white, pulpy filaments, the upper extremities of which adhere to round tubercles. In several provinces of Egypt these tuber-

\* Willdenow in his *Species Plantarum* does not speak of this nymphæa.  
cles,

cles, and particularly those of the first species, are plucked up after the inundation: they are used as food; they have an insipid earthy taste, and form a delicate dish.

The leaves of both are large, pretty numerous, sub-orbicular, divided into two lobes from their base to the place where the petiole is inserted; that is to say, nearly to the middle of the disk. They are not so thick as those of our nenuphars in Europe: they have a darker green colour, and a shining appearance above; and below are often tinged with purple or violet. These leaves are supported by radical cylindric petioles, slightly compressed and sometimes very long, which float on the surface of the water.

The flowers are very large, and are often more than four inches in diameter. Their pedicles, which arise from the root, are uniflorous and differ very little from the petioles.

The calyx of these flowers is composed of eight leaves disposed in two rows, and coloured in the inside: the more interior ones are the most coloured, and have some resemblance to the petioles.

The latter, from twelve to twenty in number, are also disposed in several series.

The stamina are of a yellow colour with large filaments, and petaliform.

The radii of the stigma vary, for the most part, in number from twelve to twenty-five. They have the same colour as the stamina, are compressed and slightly bent at the summit.

Such is the common appearance of the roots, the leaves, and the flowers. Let us now examine the differences they exhibit.

The leaves of the *nymphaea lotus* are bordered quite round with very acute indentations, firm and almost like prickles: their lobes, for the most part, are exactly parallel and sometimes even; they converge, and mutually cover each other: their inferior surface is charged with very prominent ribs, which form a very apparent reticulation: their petioles are rough.

On the other hand, those of the *nymphaea cærulea* have their edges scarcely sinuated; their lobes are more pointed, and commonly divergent. The inferior surface exhibits ribs scarcely sensible, the principal ones of which only are somewhat prominent; the rest being concave and less elevated than the disk. The petioles of these leaves are exceedingly rough.

In the *nymphaea lotus* the leaves are always more open. The folioles of the calyx are oval, lanceolated, greenish externally,

nally, without spots, but marked with seven paler ribs very distinct.

The folioles of the calyx in the *nymphaea cærulea* are much narrower, lanceolated, and almost cuneiform: the exterior side of them is of a dark green colour, constantly varied with an infinite number of points and small lines of a purplish colour, and have no apparent ribs.

The petals of the *nymphaea lotus* are oval, lanceolated, and very unequal; those of the last being much smaller than the rest: their colour is a pure milk-white, tinged with a greenish purple colour on the outside, but rarely.

In the *nymphaea cærulea* the petals are lanceolated all perfectly equal, of a bright white colour, tinged, in particular towards the summit, with the most beautiful azure, inclining slightly however, sometimes, to violet.

The stamina of the *nymphaea lotus* have antheræ very much compressed, lanceolated, without any appendix at the summit, and have scarcely the length of the filaments.

On the other hand, the antheræ of the *nymphaea cærulea* are very little compressed, lineal and longer than the filaments; their summit is terminated by a subulated blueish appendix similar to a small petal.

In the last place, the summits of the radii of the stigma are longer and subulated in the *nymphaea lotus*; shorter, oval, and lanceolated, in the *nymphaea cærulea*.

The smell exhaled from the flowers of each kind is also very different: that of the *nymphaea cærulea* is exceedingly sweet and agreeable; that of the *nymphaea lotus* is stronger, more pungent, and much less agreeable.

In regard to the fruit, I did not find in them any very remarkable difference: in both species it consists of a dry round berry, which is long, covered by the bases of the folioles of the calyx and those of the petals; truncated and radiated at the summit, which is always stained by the decomposition of the stamina and interior petals: it is divided into several cells, each of which corresponds to a radius of the stigma, and contains a quantity of round seeds of a rose colour.

The Arabs have very well distinguished these plants, and given to each a particular name: the former they call *neoufar*, and the latter *bachenin*.

By selecting from the description here given the most prominent differences, both species may be characterized as follows:

NYMPHÆA LOTUS,

N. foliis dentatis, antheris apice simplicibus.

NYMPHÆA CÆRULEA,

N. foliis repandis, antheris apice subulato-petaloides.

*Explanation of the Plate. (VII.)*

1. A petal.
  2. A stamen of the interior series.
  3. A stamen of the exterior series.
  4. The ovarium and stigma.
  5. The seeds.
  6. A section of the pedicle.
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LX. *On Mr. GREATHEAD'S Life-Boat\*.*

SIR,

A CONSIDERABLE time has elapsed since I had the honour to lay before the Society a model of the life-boat of my invention.

I have now enclosed a particular account of its construction in a letter from Mr. Hinderwell, explaining upon what principle it is built, so as to render it superior to any other form of a boat for the dangerous enterprises for which it was intended, and has been used.

I am, Sir, your humble servant,  
HENRY GREATHEAD.

South Shields,  
Jan. 1, 1802.

*To Mr. Charles Taylor.*

SIR,

IT is much to be lamented, that in an age enlightened by science, such a languid indifference should prevail on many important public occasions; and that the most excellent inventions should have to combat the force of inveterate prejudice.

How many valuable discoveries have languished in obscurity! How many useful projects have perished in embryo, deprived of the fostering aid of the public, and the patronage of influence and authority! In the class of useful improvements for the diminution of the dangers incident to a maritime profession, the life-boat, invented by Mr. Greathead, of Shields, has a claim to a distinguished patronage. An experimental conviction of its great utility in saving the lives of shipwrecked seamen, and of its perfect safety in the most agitated sea, has induced me to advocate the cause with a

\* From the *Transactions of the Society for the Encouragement of Arts, &c.* for 1802.—The Society's gold medal and fifty guineas were voted to Mr. Greathead for this invention.

zeal proportioned to its importance; and it is a consolatory reflection to my own mind, that my exertions have been successful in the introduction of a life-boat in the port of Scarborough, and, I trust, not unprofitable towards promoting a similar establishment in other places. The services which have been recently performed at this port by means of the life-boat, in contributing to the preservation of the lives of the crews of two vessels, more than compensate for every labour. I am far from the ambition of aspiring to any honorary testimony on this occasion. Actuated by the purest principle of philanthropy, my sole object is the benefit of the community, and to endeavour, by ardent recommendations, to excite a spirit of emulation in order to introduce the life-boat, with its invaluable properties, into more general use. I am induced to submit, with the utmost deference and respect, to the consideration of the Society of Arts, &c. the following description of the life-boat, with some miscellaneous observations. The construction of the boat, agreeably to Mr. Greathead's plan, is as follows:

The length is thirty feet; the breadth, ten feet; the depth, from the top of the gunwale to the lower part of the keel in midships, three feet three inches; from the gunwale to the platform (within), two feet four inches; from the top of the stems (both ends being similar) to the horizontal line of the bottom of the keel, five feet nine inches. The keel is a plank of three inches thick, of a proportionate breadth in midships, narrowing gradually toward the ends to the breadth of the stems at the bottom, and forming a great convexity downwards. The stems are segments of a circle, with considerable rakes. The bottom section, to the floor-heads, is a curve fore and aft, with the sweep of the keel. The floor-timber has a small rise curving from the keel to the floor-heads. A bilge-plank is wrought in on each side next the floor-heads with a double rabbet or groove of a similar thickness with the keel; and on the outside of this are fixed two bilge-trees, corresponding nearly with the level of the keel. The ends of the bottom section form that fine kind of entrance observable in the lower part of the bow of the fishing-boat, called a *coble*, much used in the North. From this part to the top of the stem it is more elliptical, forming a considerable projection. The sides, from the floor-heads to the top of the gunwale, flaunch off on each side, in proportion to about half the breadth of the floor. The breadth is continued far forward towards the ends, leaving a sufficient length of strait side at the top. The sheer is regular along the strait side, and more elevated towards the ends. The  
gunwale,

gunwale, fixed on the outside, is three inches thick. The sides, from the under part of the gunwale along the whole length of the regular sheer, extending twenty-one feet six inches, are cased with layers of cork to the depth of sixteen inches downward; and the thickness of this casing of cork being four inches, it projects at the top a little without the gunwale. The cork on the outside is secured with thin plates or slips of copper, and the boat is fastened with copper nails. The thwarts, or seats, are five in number, double banked, consequently the boat may be rowed with ten\* oars. The thwarts are firmly stanchioned. The side oars are short†, with iron tholes and rope grommets, so that the rower can pull either way. The boat is steered with an oar at each end; and the steering oar is one-third longer than the rowing oar. The platform placed at the bottom within the boat is horizontal, the length of the midships, and elevated at the ends for the convenience of the steersman, to give him a greater power with the oar. The internal part of the boat next the sides, from the under part of the thwarts down to the platform, is cased with cork; the whole quantity of which affixed to the life-boat is nearly seven hundred weight. The cork indisputably contributes much to the buoyancy of the boat, is a good defence in going alongside a vessel, and is of principal use in keeping the boat in an erect position in the sea, or rather of giving her a very lively and quick disposition to recover from any sudden cant or lurch which she may receive from the stroke of a heavy wave. But exclusive of the cork, the admirable construction of this boat gives it a decided pre-eminence. The ends being similar, the boat can be rowed either way; and this peculiarity of form alleviates her in rising over the waves. The curvature of the keel and bottom facilitates her movement in turning, and contributes to the ease of the steerage, as a single stroke of the steering oar has an immediate effect, the boat moving as it were upon a centre. The fine entrance below is of use in dividing the waves, when rowing against them; and, combined with the convexity of the bottom and the elliptical form of the stem, admits her to rise with wonderful buoyancy in high sea, and to launch forward with rapidity, without shipping any water, when a common boat would be in danger of being filled. The flaunching, or spreading form of the boat, from the floor-heads to the gunwale, gives her a considerable bearing; and the continuation

\* Five of the benches are only used, the boat being generally rowed with ten oars.

† The short oar is more manageable in a high sea than the long oar, and its stroke is more certain.

of the breadth, well forward, is a great support to her in the sea; and it has been found by experience, that boats of this construction are the best sea-boats for rowing against turbulent waves. The internal shallowness of the boat from the gunwale down to the platform, the convexity of the form, and the bulk of cork within, leave a very diminished space for the water to occupy; so that the life-boat when filled with water contains a considerable less quantity than the common boat, and is in no danger either of sinking or overturning. It may be presumed by some, that in cases of high wind, agitated sea, and broken waves, that a boat of such a bulk could not prevail against them by the force of the oars; but the life-boat, from her peculiar form, may be rowed a-head when the attempt in other boats would fail. Boats of the common form, adapted for speed, are of course put in motion with a small power, but for want of buoyancy and bearing are overrun by the waves and sunk, when impelled against them; and boats constructed for burden meet with too much resistance from the wind and sea when opposed to them, and cannot in such cases be rowed from the shore to a ship in distress. An idea has been entertained, that the superior advantages of the life-boat are to be ascribed solely to the quantity of cork affixed. But this is a very erroneous opinion; and, I trust, has been amply refuted by the preceding observations on the supereminent construction of this boat. It must be admitted, that the application of cork to common boats would add to their buoyancy and security; and it might be a useful expedient, if there were a quantity of cork on board of ships, to prepare the boats with in cases of shipwreck, as it might be expeditiously done, in a temporary way, by means of clamps, or some other contrivance. The application of cork to some of the boats of his majesty's ships\* might be worthy of consideration; more particularly as an experiment might be made at a little expense, and without inconvenience to the boats; or may prevent pleasure boats from upsetting or sinking.

The life-boat is kept in a boat-house, and placed upon four low wheels, ready to be moved at a moment's notice. These wheels are convenient in conveying the boat along the shore to the sea; but if she had to travel upon them on a rough road, her frame would be exceedingly shaken. Besides, it has been found difficult and troublesome to replace her upon these wheels on her return from sea. Another plan has therefore been adopted. Two wheels, of nine feet dia-

\* The launches.

meter, with a moveable arched axis, and a pole fixed thereto for a lever, have been constructed. The boat is suspended near her centre between the wheels under the axis, toward each extremity of which is an iron pin with a chain attached. When the pole is elevated perpendicularly, the upper part of the axis becomes depressed, and the chains being hooked to eye-bolts on the inside of the boat, she is raised with the utmost facility by means of the pole, which is then fastened down to the stem of the boat.

The Scarborough boat is under the direction of a committee. Twenty-four fishermen, composing two crews\*, are alternately employed to navigate her. A reward, in cases of shipwreck, is paid by the committee to each man actually engaged in the assistance; and it is expected that the vessel receiving assistance should contribute to defray this expense. None have hitherto refused.

It is of importance that the command of the boat should be intrusted to some steady, experienced person, who is acquainted with the direction of the tides or currents, as much skill may be required in rising them to the most advantage in going to a ship in distress. It should also be recommended, to keep the head of the boat to the sea as much as circumstances will admit; and to give her an accelerated velocity to meet the wave. Much caution is necessary in approaching a wreck, on account of the strong reflux of the waves, which is sometimes attended with great danger. In a general way, it is safest to go on the lee quarter; but this depends upon the position of the vessel; and the master of the boat should exercise his skill in placing her in the most convenient situation. The boatmen should practise themselves in the use of the boat, that they may be the better acquainted with her movements; and they should at all times be strictly obedient to the directions of the person who is appointed to the command.

The great ingenuity which has been displayed in the construction of the life-boat, leaves scarcely any room for improvement; but some have supposed that a boat of twenty-five feet in length, with a proportionate breadth, would answer every purpose of a larger one. A boat of these dimensions would certainly be lighter, and less expensive; but whether she would be equally safe and steady in a high sea, I cannot take upon myself to determine.

Mr. Greathead, of South Shields, the inventor, under-

\* Two crews are appointed, that there may be a sufficient number ready in case of any absence.

takes to build these boats, and to convey them to any port in the kingdom. He is a worthy man, in whom a confidence may be reposed, and will build upon moderate terms of profit.

THOMAS HINDERWELL.

*Description of the Life-Boat.*

EEE (Plate VIII.), the sheer or curve of the boat.

II, the two stems or ends.

K, the keel.

LL, the aprons, to strengthen the stems.

MM, the sheets, or places for passengers.

NN, timber-heads, or boat-fastenings.

OOOOO, the tholes on which the oars are slung by grommets.

T, flooring under the rowers feet.

Fig. 2. a cross section of the life-boat.

FF, the outside coatings of cork.

GG, the inside cork filling.

HH, the outside planks of the boat.

I, one of the stems of the boat.

K, the keel.

NN, the timber heads.

P, the thwarts, or rowers seats.

R, one of the stanchions under the thwarts, each being thus firmly supported.

S, a section of the gang-board, which crosses the thwarts, and forms the passage from one end of the boat to the other.

T, the floor-heads, or platform for the rowers feet.

VV, the two bilge-pieces, nearly level with the keel.

WW, the gunwales.

X, a ring-bolt for the head-fast, there being another also at the other end.

Y, platform for the steersman.

Fig. 3. a truck or carriage with four wheels, to convey the boat to and from the sea.

a, an oblong frame of wood consisting of two long pieces, hollowed a little to admit the body of the boat, and secured by the cross pieces bb.

cccc, four low wheels, each sunk or hollowed in the middle, to run better upon a rail-way or timber-road.

dd, two indents made in the side timbers, that the bottom of the boat may lie firm therein.

ee, two small rollers, moveable in the cross timbers for the keel of the boat to slide upon.

ff, the

ff, two long rollers, one at each end of the frame, to assist in raising the boat upon or sliding it off the truck or carriage.

*Management of the Life-Boat from the Boat-House to the Sea, and vice versa, as practised at Lowestoffe, in Suffolk.*

The life-boat may be launched from any beach, when wanted, with as much ease as any other boat, by proper assistance. The distance from the boat-house at Lowestoffe to the shore is one hundred yards, and the boat's crew can run her down in ten minutes. When the sea does not tumble in upon the beach very much, the boat may be easily launched by laying the ways as far as possible in the water, and hauling the carriage from under her.

When there is a great sea on the beach, the boat must be launched from the carriage before she comes to the surf, on planks laid across, as other boats are launched; the people standing on the ends to prevent the sea moving them; then, with the assistance of the anchor and cable (which should be laid out at sea for the purpose), the boat's crew can draw her over the highest sea.

Upon the boat returning to the shore, two double blocks are provided; and, having a short strop fixed in the hole, in the end of the boat next the sea, the boat is easily drawn upon the carriage. The boat's crew can run her any distance upon a clear shore by the carriage of Mr. Greathead's contrivance.

*Account of, and Instructions for, the Management of the Life-Boat.*

The boats in general of this description are painted white on the outside, this colour more immediately engaging the eye of the spectator at her rising from the hollow of the sea than any other. The bottom of the boat is at first varnished (which will take paint afterwards); for the more minute inspection of purchasers. The oars she is equipped with are made of fir of the best quality, having found by experience that a rove-ash oar that will dress clean and light is too pliant among the breakers; and when made strong and heavy, from rowing double banked, the purchaser, being short, sooner exhausts the rower, which makes the fir oar, when made stiff, more preferable.

In the management of the boat, she requires twelve men to work her; that is, five men on each side, rowing double banked, with an oar slung over an iron thole, with a grommet (as provided) so as to enable the rower to pull either way;

way; and one man at each end to steer her, and to be ready at the opposite end to take the stern oar when wanted. As, from the construction of the boat, she is always in a position to be rowed either way, without turning the boat, when manned, the person who steers her should be well acquainted with the course of the tides, in order to take every possible advantage: the best method, if the direction will admit of it, is to head the sea. The steersman should keep his eye fixed upon the wave or breaker, and encourage the rowers to give way as the boat rises to it; being then aided by the force of the oars, she launches over it with vast rapidity, without shipping any water. It is necessary to observe, that there is often a strong reflux of sea, occasioned by the stranded wrecks, which requires both dispatch and care in the people employed, that the boat be not damaged. When the wreck is reached, if the wind blows to the land, the boat will come in shore without any other effort than steering.

I would strongly recommend practising the boat, by which means, with experience, the danger will appear less, from the confidence people will have in her from repeated trials.

LXI. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them.* By C. LEFEBVRE, Member of the Council of Mines, of the Philomatic Society, &c. &c.

[Continued from p. 240.]

*Department of Ille and Vilaine.*

NO coals are dug up in this department. It may receive this fuel by sea in the northern part; and the mines of Montrelais and North, in the department of Loire-Inferieure, may supply the southern part.

*Department of Indre.*

This department is in the same situation as the preceding in regard to the want of coal mines. It might be supplied from the mines in the department of La Creuse, if the river of that name, which is navigable for boats only to Argentan, were rendered navigable higher up.

If the navigation of the Cher were improved, it might also supply with coals the eastern part of the department of Indre, because the coal mines in the environs of Montluçon would then become an object of importance.

*Department*

*Department of Indre and Loire.*

There are no coal mines in this department; but it receives the products of the mines of various departments by the river Loire, which traverses it in its longest direction.

*Department of Isere.*

The southern part of this department (22) contains some coal mines; and particularly in the environs of the communes of La Motte, Pierre-Chatel, La Mure, Saint-Barthelemy-de-Sechilienne, &c.

They supply a mineral combustible of a moderate quality; but is exceedingly valuable in a country where wood is daily becoming scarcer.

These coal mines are almost all worked in an irregular manner, without attention to the lives of the workmen or to economy.

The annual product of these mines may be estimated at from a million to twelve hundred myriagrammes.

The price at the mines is from 15 to 20 cents per myriagramme; but the difficulty of land-carriage raises the price at Grenoble to from 60 to 80 cents.

*Department of Jura.*

Several indications of coals have been announced in this department; and some attempts were made to work mines, which gave rise to well-founded hopes: but these attempts are now abandoned, and no mines are worked in this department.

In the present state of things, the mines of Blanzey and Saint-Berain, in the department of Saône and Loire, furnish coals to this department by the canal of Charolois and Doubs. The southern part is supplied from the mines of Rives-de-Gier.

*Department of Landes.*

Indications of coal have been announced in the neighbourhood of Dax; but from the nature of the soil it is probable that it is fossil wood. As this department has no coal mines worked, it is supplied with this fuel by sea, and particularly from the port of Bayonne.

*Department of Loire and Cher.*

This department (23) has no coal mines. It receives this fuel by the river Loire from the mines of Haute-Loire and Allier.

*Department of Haute-Loire.*

Very abundant coal mines are worked in the cantons of Brassac-Sainte-Florine, Freugères, Vergongheon, and Lempdes, the product of which is considerable. That of Grosmenil, situated in the last-mentioned commune, which had been perforated by a number of small pits in such a manner that the strata of coal was inundated, is now in the hands of a company, who are clearing it of water, and making preparations for working it to a greater depth.

There is reason to think, that this mine alone, when in a productive state, will furnish as much as all the other mines of the country do at present. The annual product, however, of the latter is from fifteen to eighteen hundred thousand myriagrammes. These coals are of an excellent quality. The means of their conveyance are the navigation of the Allier and the Loire, the canal of Briare and of the Seine; which gives for their circulation an extent of more than 140 leagues. A large quantity of these coals is consumed at Paris. The mean price of them at the pits is from 15 to 20 cents, and when delivered at Paris from 30 to 40 cents per myriagramme.

*Department of La Loire.*

The south-east part of this department (24) contains a great number of coal mines, worked in an extent of more than ten miles in length and five or six miles in breadth. The principal communes in which these mines are situated are those of Rives-de-Gier, Saint-Chamond, Saint-Etienne, Le Chambon, Firmini, Roche-Moliere, &c.

In consequence of the number and richness of the strata found in these cantons, an immense quantity of this fuel has been extracted for several centuries past; but in order to obtain it sooner, and with more ease, a great many pits have been sunk, by means of which the coals nearest the surface were dug out. The whole country is perforated with these apertures. This irregular method rendered the working of the inferior strata much more laborious and expensive.

The present product of the different mines in this department are stated at thirty millions of myriagrammes annually; and this, in all probability, is below the truth. If the mines were better worked, it is certain that the quantity might be quadrupled.

The quality and price of these coals is exceedingly various. Those of the first quality cost at the mine from 10 to 12 cents  
per

per myriagramme; those of mean quality are sold for 7 or 8 cents, and the worst for 5.

The medium price of these coals transported to Lyons is 15 or 18 cents per myriagramme: the price at Marseilles is 35 cents.

*Department of La Loire Inferieure.*

This department (25), as has been seen, may receive coals from the mines situated towards the upper part of the course of the Loire as well as from those of Allier. The coals of the mine of Decise, which will be mentioned in speaking of the department of La Nièvre, are also conveyed to different places on the Loire. The coals also of the mines of Montre-lais, situated two or three leagues to the north of Varades and Ingrande, are conveyed on the lower part of the Loire. They are embarked in particular at the latter place to be transported to the communes on the banks of that river descending to Nantes, where the consumption is considerable.

The coals annually extracted from these mines may be safely estimated at a million of myriagrammes. They are of a good quality. The mean price on the spot is 5 cents per myriagramme; when sent to Nantes, the same quantity costs 25 cents.

In this department there are also peat-mosses, the product of which is abundant, and of great utility to the inhabitants. The most considerable are found in the marshes of Montoir, to the north of Nantes. More than eight thousand individuals are employed in digging peat from these mosses.

*Department of Loire and Cher.*

There are no mines worked in this department. It receives coals by the Loire, which traverses it; and might consume those also brought by the Cher from the mines situated in the environs of Commentry and Montluçon, in the department of Allier, if the navigation of the Cher were improved.

*Department of Loiret.*

There are no coal mines worked in this department; but it is abundantly provided with coals by the navigation of the Seine.

*Department of Lot.*

Abundant coal mines exist in the environs of Figeac, towards the east extremity of this department. They are badly worked by the proprietors of the ground; but they are susceptible of improvement, if means of consumption were opened for their products.

*Department of Lot and Garonne.*

No coal mines are worked in this department. Those extracted from the mines of Carmeaux, in the department of Tarn, are brought to it on the Garonne; and it receives by the Lot those of the department of that name, which would be supplied much cheaper if the Lot were rendered navigable towards Figeac and the department of Aveyron.

*Department of La Lozere.*

The discovery of some strata of coal in this country would be of great utility. Wood daily becomes scarcer; and the communication with the coal countries in the neighbouring departments is difficult and expensive.

Several indications of coal have been announced; particularly towards Canourgue, towards Mende, and in the neighbourhood of Meyrmeu. Some specimens sent to the Council of Mines announce only fossil wood.

*Department of Lys.*

This department has no coal mines worked. It receives those of the departments of the North and of Jemappes.

*Department of La Manche.*

There are several indications of coal in this department; and particularly in the Forest of Briquebec, near Valonges; in the commune of Pleffis, near Fretot; in that of Moon, and that of Semilly, in the district of Saint-Lô.

Strata of coal have been found in the commune of Pleffis; but hitherto they are so intermixed with strata of schist, that if worked they would not pay the expenses.

*Department of La Marne.*

This department, as far as is yet known, has no coal mines. Collections of fossil wood, and of peat exceedingly pyritous, are frequently found under strata of marly earth. These substances have often been announced as coal, but they do not possess the qualities of that fuel. They kindle slowly, and become totally incandescent; but they give very little flame, and for the most part none at all. This substance is called by the inhabitants *earth coal*.

The Valley of La Vesle furnishes abundance of peat of an excellent quality. This river, which takes its source on the east of Chalons, passes Rheims and Braine, and discharges itself into the Aisne above Soissons, traversing an extent of eighteen leagues. It every where runs over a bed of peat,  
and

and may afford great resources to the neighbouring communes if that substance were properly dug up.

*Department of La Haute Marne.*

There are no coal mines worked in this department. Several indications of coal have been announced, but hitherto the specimens have turned out to be bituminous fossil wood. This department is at a distance from coal countries, and has on easy communication with them; but it is abundant in wood.

*Department of La Mayenne.*

This department is supplied with coals by the Loire. Boats can go up the Mayenne as far as Laval; and the Sarthe as far as Mons.

*Department of Mayenne and Loire.*

There are several small coal pits worked in the canton (26) of Saint-Aubin-de-Luigné, in the territories of Chaudefond, Montjean, and several other places adjacent. They are worked very irregularly, and the product of them is not known.

The mine of Saint-George's-Châteloison, situated between Vihiers and Doué, to the east of the latter commune, is more important and worked in a more regular manner. The product amounts to about 300,000 myriagrammes per annum; but it might be rendered more considerable.

*Department of La Meurthe.*

A discovery of coal has been announced in the neighbourhood of Nancy; but the specimens sent were only bituminous fossil wood. At present this department has no coal mines actually worked.

*Department of the Meuse.*

No coal mines are worked in this department (27). It can receive coals from La Saare by the Moselle, and the canal which communicates with the Meuse between Toul and Pagny: and the coals of the department of Ourthe may be conveyed on the Meuse to its northern part.

*Department of La Meuse Inferieure.*

This department possesses very important coal mines in the neighbourhood of Rolduc. The annual product of them amounts to more than 13,500,000 myriagrammes; and they are far from being properly worked.

The coals are of different qualities; some of them are very good. Their mean price at the mine varies from 5 to 14 cents

per myriagramme. There is also abundance of peat in the cantons of Heythnyfen and De Weert.

*Department of Mont-Blanc.*

There are several coal mines in this country (28). Some are worked in the territory of the communes of Entrevernes near Annecy, Montmin, Novalaise, Servolex, and Petit-Bernard. Indications of coal have been announced also in the canton of Moutiers, Cruseilles, Valloires, and Cognin. The annual product of the coal mines worked in this department may be estimated at 120,000 myriagrammes per annum. These mines are susceptible of improvement, but there is no consumption. The price of the coals at the mine is 5 cents per myriagramme.

*Department of Mont-Tonnere.*

There are more than thirty coal mines known in this department (29). Several of them have been abandoned in consequence of the war. The cantons most abundant are those of Lautereck, Wolfstein, and Obermoschel.

The product of these mines may be estimated at about 425,000 myriagrammes; but if the consumption required, it could be increased. The quality of the coal varies. There is very little of it good; but it is generally used for heating stoves. The price at the mines is 8 cents per myriagramme.

These coals are consumed merely by the wants of the country. A considerable quantity of them is employed for burning lime, either for building or for manure. They are employed also in the fusing of mercury, of which there is very important mines in this department; and for the purposes of evaporation in the salt-works of Kreutznach.

*Department of Morbihan.*

The Morbihan has no coals. It receives those which are dug up on the banks of the Loire, or of the rivers united with it.

*Department of La Moselle.*

Some coal mines are worked in this department (30) in the environs of the communes of Ostenbach and in the canton of Petelange. The annual product of them may be estimated at 100,000 myriagrammes at least. The coal is of a very good quality. It costs at the mine 9 cents per myriagramme.

*Department of the Two Nethes.*

This department has no coal mines; but it is supplied with

with abundance of coal from the mines in the departments of Jemappes and of the North, by the canal of Brussels and by the Scheldt.

*Department of the North.*

Very important coal mines are worked at Anzin near Valenciennes, at Fresnes, Raismes, and Vieux-Condé. There is a very considerable one also in the commune of Aniche.

The different mines worked in this department furnish at the least 30,000,000 of myriagrammes. They are of various qualities. Some of them are very good for forging iron; others are preferable for stoves; and some of an inferior quality are proper for burning lime. The price at the mines differs according to the quality. The mean price of good coal is from 12 to 15 cents per myriagramme. When delivered at the ports of Ostend, Dunkirk, and Calais, it costs from 25 to 28, and at Havre from 52 to 55.

The means of consumption for these coals, and particularly for those near Condé and Saint-Amand, are very extensive towards the north, on account of the navigation of the Scheldt, and of the numerous canals which communicate with that river. So that the products of these mines might be conveyed at very little expense as far as Ghent, Bruges, Ostend, Termonde, and Antwerp. They might also be circulated in Holland, and be exported by the ports of Ostend and Dunkirk, so as to become as important an object of maritime commerce as the English coals.

*Department of La Nièvre.*

Coal mines are worked in the canton of Decize (32), and are very productive. The annual product is estimated at 1,000,000 of myriagrammes, but may be much increased. The quality of these coals is in general such that they must be speedily employed after they are dug up, as they lose considerably by long exposure to the air. The price at the mine is from 8 to 10 cents per myriagramme; and delivered at Paris from 10 to 13. They are conveyed on the Loire, the canal of Briare, the Seine, &c.

[To be continued.]

LXII. *Memoir on the Stones said to have fallen from the Heavens. Read in the French National Institute by C. VAUQUELIN* \*.

WHILE all Europe resounded with the report of stones fallen from the heavens, and while philosophers, divided in opinion on this subject, were forming hypotheses to explain the origin of them, each according to his own manner, Mr. Edward Howard, an able English chemist, was pursuing in silence the only route which could lead to a solution of the problem. He collected specimens of stones which had fallen at different times, procured as much information as possible respecting them, compared the physical or exterior characters of these bodies; and even did more, in subjecting them to chemical analysis by means as ingenious as exact.

It results from his researches, that the stones which fell in England, in Italy, in Germany, in the East Indies, and in other places, have all such a perfect resemblance that it is almost impossible to distinguish them from each other; and what renders the similitude more perfect and more striking is, that they are composed of the same principles and nearly in the same proportions.

Before the last results of the labour of Mr. Howard were known in France, I had employed myself on the same object; and I have the satisfaction to find in his memoir, which has been since printed, that they perfectly agree with those which I had obtained.

I should have abstained from any public notice of an object which has been treated of in so able a manner by the English chemist, if he himself had not induced me to do so during his residence at Paris; had not the stones which I analysed been from another country; and had not the interest excited by the subject rendered this repetition excusable.

It is therefore to gratify Mr. Howard, to give, if possible, more weight to his experiments, and to enable philosophers to place full confidence in them, rather than to offer any thing new, that I publish this memoir. One of the stones which I examined was transmitted to me by C. Saint-Amans: it fell at Créon, in the parish of Juliac, on the 24th of July 1790, about nine in the evening. This stone appeared in the air under the form of a fire-ball, which was visible in almost the whole of the south of France. A very

\* From the *Journal des Mines*, No. 76.

correct account was given of it at that time in the abbé Bertholon's *Journal d'Histoire Naturelle*, together with the proces-verbal of the municipality of the place, which confirmed the fall of this stone.

Another stone was given to me by M. Darcet junior: it fell at Barbotan, near Roquefort, in the month of July 1789. The brother of the late Darcet, curé in the neighbourhood, sent it to him, with the proces-verbal drawn up respecting this extraordinary phænomenon. C. Lomet, who is known to several members of the Institute, was at Agen on the day when this kind of meteor appeared in the atmosphere. The following is the account which he gave me of it:

“ It appeared as a very bright fire-ball, the light of which was as pure as that of the sun; it had the size of a common air-balloon, and was long enough visible to throw the inhabitants of the country into the greatest consternation; after which it burst, and disappeared. A few days after, some peasants brought stones, which they said were the result of the fall of the meteor: but at that period they were laughed at. What they said was considered as fables; and those to whom the stones were offered, would not accept of them. The peasants would have now more reason to laugh at philosophers.”

The third kind of these stones is that brought from Benares in the East Indies, which fell on the 19th of December 1798, exhibiting the very same phænomena as were observed under similar circumstances in other countries. It was given to me by C. De Drée and by C. Saint-Amans, who brought it from England.

All these stones have a similar appearance, and one might readily believe that they had been detached from the same mass. Their surface is blackish, smooth, and, as it were, varnished by a commencement of fusion. The inside is of a whitish gray colour marked with a greater or less number of brown spots, or spots of a darker gray colour than the rest of the mass. Those, however, found at Benares, and in Yorkshire, are whiter in the interior part than those found in France. There are observed in them white pyrites, the fracture of which is very much lamellated; globules of metallic and ductile iron, some of which weigh 46 English grains; but this iron has a whiter colour, and a greater degree of hardness, than common iron. The cause of this difference will be seen hereafter.

I wished to have been able to subject to a separate analysis each of the constituent principles of these stones sensible to the eye: but they are so intimately mixed, that it was impossible to separate them exactly. By patience, however,

I was able to obtain a sufficient quantity of the globules of iron and of pyrites to ascertain the nature of them.

A. A hundred parts of the stones of Benares, pulverized in a mortar of hard stone, and sifted through a fine silk sieve to separate the coarsest particles of the iron which cannot be pulverized, were treated with dilute nitric acid. During this operation there was disengaged a large quantity of nitrous gas; the acid assumed a yellowish green colour; the stony powder became whitish, and, dividing itself, increased considerably in volume, so that it resembled gelatinous silex. Sulphureous particles were observed floating at the surface of the liquor.

B. When the action of the nitric acid ceased, water was added to the mixture; the liquor being filtered, the undissolved matter was washed, and after desiccation in the open air it weighed no more than 64 parts.

C. As the matter treated in this manner was still slightly coloured, it was boiled with muriatic acid, which, as is well known, unites more easily with oxidated iron: it assumed also a greenish yellow colour, and the powder became much whiter. After this second operation the stone, when washed and calcined in a platina crucible, weighed only 47 parts.

D. The nitric and muriatic acids which had been successively poured over the stone as above mentioned were mixed together and then precipitated by ammonia, of which an excess was added. Being heated for some time, the liquor was filtered, and the precipitate was washed and calcined: it had a brown colour, and weighed 38 parts.

E. The ammoniacal liquor had a slight blue colour inclining to violet. Alkaline carbonates produced in it no precipitate; on the other hand, caustic alkalies formed a white precipitate very abundant, but the liquor lost none of its violet colour. This precipitate, when washed and dried in the air, had a greenish shade, and weighed 18 parts; calcination reduced it to 13. This matter readily combined with sulphuric acid, and the result was a solution, which by spontaneous evaporation produced prismatic crystals resembling in taste and flavour those of the sulphate of magnesia. It however had a slight greenish colour, and at the end of some minutes produced in the mouth a metallic flavour. To know whether the colour and flavour of the sulphate of magnesia arose from some metallic substances it was dissolved in water, and hydrosulphuret of ammonia being poured over it, a pretty voluminous black precipitate was produced. After washing and desiccation, however, it weighed only two parts. We shall return hereafter to this matter.

F. The

F. The ammoniacal liquor, from which the magnesia had been separated by means of caustic potash, was mixed with a solution of sulphurated hydrogen. By this mixture there was formed a very voluminous black precipitate, which when washed and dried weighed seven parts. This part being united with the three parts obtained also by sulphate of magnesia, and kept at a red heat for some minutes, exhaled the odour of sulphureous acid, and acquired a very dark green colour inclining to brown: the whole then weighed scarcely three parts.

A small quantity of this matter fused with borax gave glass of a hyacinth colour; put into sulphuric acid diluted with water, it dissolved only in part, and a small quantity which had a metallic appearance refused to combine with it; but the addition of a few drops of nitric acid, by oxidating it, favoured its solution: by spontaneous evaporation it furnished elongated crystals of a very beautiful green colour.

By the properties which this matter exhibited it is evident that it was nothing else but oxide of nickel; for it is the only one among the metals which possesses the property of colouring borax of a hyacinth red colour by fusion, of giving to ammonia a purplish blue colour, of forming with sulphuric acid a prismatic salt of a green colour, and of producing by its combination with acids and ammonia triple salts which are not precipitated by fixed alkalies.

An examination of the principles successively obtained by the different means employed for analysing the stone of Benares, proves that it is composed of silice, magnesia, and iron, a part of which appears to be oxidated, nickel and sulphur. Iron, nickel, and sulphur, according to every appearance, form a particular triple combination, which seems only to be interposed between the earthy parts. I am, however, inclined to believe, that one part at least of these substances is really in combination with the earths; for at the moment when the stone is attacked by the acids, the silice shows itself in a state of very great division, and like a kind of jelly. The proportions in which these matters are found in the stone of Benares are nearly as follows:

Silice	48
Oxidated iron	38
Magnesia	13
Nickel	3
Sulphur an indeterminate quantity	

If we might suppose that the iron contained in this stone exists in the perfect metallic state, we might easily find, by the increase of weight which it acquires in consequence of oxygenation, the proportion of sulphur; but as it is certain that this metal is oxidated in it in an unknown quantity, that of the sulphur cannot be determined.

A hundred parts of this stone, freed as far as could possibly be done from ferruginous matters, were treated with muriatic acid diluted with a little water. The mixture produced immediately, with a strong effervescence, a gas which had the odour of sulphurated hydrogen gas. The matter assumed the form of a gelatinous mass, which seems to prove, as already said, that the *silex* exists in this stone in a state of combination either with iron or with other substances.

When the effervescence had subsided, and the mixture had boiled for some time, it was diluted with water and filtered. The liquor had a green colour analogous to that of muriate of iron, but a little darker. The residuum when washed was white, and weighed 49 parts.

The muriatic solution and washings being united together were precipitated by ammonia added in excess, and were stirred for some time with the precipitate.

It has been remarked, that oxidated iron, precipitated in this manner by ammonia, has a more intense colour than that of iron obtained under the same circumstances. This iron, washed and dried, weighed 42 parts.

The liquor containing the superabundant ammonia in the precipitation of the iron had acquired a violet colour, which neither the heat nor the contact of the air made it lose. Alkaline carbonates produced in it no alteration; on the other hand, fixed caustic alkalies formed in it a white precipitate pretty abundant, which when washed and calcined weighed twelve parts. This matter combined with sulphuric acid gave sulphate of magnesia, coloured green by a small quantity of nickel which the magnesia had carried down with it.

The liquor from which this earth had been separated by potash had lost a part of its violet colour; it, however, still formed a black precipitate with sulphurated hydrogen. From what I had before observed, I entertained no doubt that the substance which coloured the magnesia green, and was precipitated black by hydro-sulphuret of ammonia, was nickel. I therefore separated, by means of this reagent, the portion which remained in solution and that which was mixed with the sulphate of magnesia: I united them, and having calcined them to separate the sulphur, I obtained a green oxide, which weighed three parts and a half.

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The method employed to separate the iron and the magnesia is founded on this circumstance, that a solution of that earth containing an excess of acid is not precipitated by ammonia, because there is formed a triple salt, which cannot be decomposed by a superabundance of that alkali; but the acid must be in such quantity that the salt resulting from its combination with the ammonia may be able to saturate the salt of magnesia existing in the liquor. Care was taken to observe this.

Alkaline carbonates produce no precipitation of the substances which enter into combination with the triple salt here mentioned, though it must, however, be decomposed; but there is formed another kind, composed of magnesia, carbonic acid, and ammonia, which remains in solution. But one thing I did not foresee was the simultaneous precipitation of a portion of nickel and magnesia by the caustic potash; for, as Mr. Howard remarked, this metal is found there in complex combination, the oxide of which ought to be dissolved by the ammonia in proportion as it becomes free: the precipitation of this metal must be produced by its affinity for magnesia, at least this is what appears most probable.

After these two analyses of the stone of Benares, and the conformity of the results which they furnished, though effected by methods somewhat different, there can be no doubt that it is really composed of iron, siliceous earth, magnesia, nickel, and sulphur, as Mr. Howard announced.

Having therefore confirmed by these two means, and by others, which it is needless to mention, the results given by the English chemist in regard to the composition of the stone of Benares, I subjected to similar experiments two other kinds of stone which fell in France, in order to ascertain whether they would furnish the same principles of analysis as their external characters seemed to announce.

To avoid a repetition of the details already given of the analytical methods employed, I shall only observe, that the stones which fell at Barbotan and Juliac, treated with sulphuric, nitric, muriatic, &c. acids, exhibited in the series of operations to which they were subjected, and by the nature of the elements which they furnished, the most perfect similarity to the preceding. It may therefore be considered as fully proved, that the stones said to have fallen from the clouds in different countries are composed of principles perfectly similar, and consequently that we must have recourse to a common cause to explain their formation, and to suppose also a common source from which nature derived the elements of them. I must, however, acknowledge, that the  
specimens

specimens of the stones which have fallen in France are a little more charged with iron than those of other countries; but as this metal exists in them, for the most part, in distinct globules which cannot be pulverized, they could have no influence on the results of the analysis, as the earthy part was sifted through a fine sieve.

All the stones which have fallen to the earth, and particularly in France, contain, as I have already mentioned several times, globules of iron in a metallic state more or less distinct, some of which weigh from forty to sixty English grains. It appeared to me of importance to subject this iron to some analytical trials in order to ascertain the nature of their constituent principles; but before I give the results it may be of utility that I should first give an account of their physical characters. These globules are much whiter than common iron; their colour approaches that of tin; their hardness is also greater, and consequently they are more difficult to be forged.

This metal dissolves easily, and with effervescence, in all those acids which dissolve common iron; but instead of giving pure hydrogen gas, it furnishes hydrogen gas very sensibly sulphurated. A part only of this hydrogen gas, indeed, is combined with the sulphur; for, having made it to pass through water and caustic alkalies, the greater part of this fluid was not dissolved; and after having made the different liquids here mentioned to pass in the same manner, no sensible signs of sulphur were given by reagents; but the water and the alkalies were manifestly hydro-sulphurated, since they then precipitated black the greater part of metallic solutions, and particularly lead. This sulphurated hydrogen gas exhibited to me a phenomenon which, as far as I know, has never been before observed in its combination with water—I mean a very speedy decomposition which it experienced in a flask perfectly closed in which it had been preserved for some days. At the end of that time there were at the bottom of the water a great many small white laminæ, and the water had no odour: it no longer precipitated solutions of lead. This gas then had experienced complete decomposition.

The solution of this iron in muriatic acid was precipitated by ammonia, of which a superabundance was added. The liquor when filtered had a colour inclining to purple; the oxide of iron, when washed and calcined, was of a brown colour, and had sensibly increased in weight. The ammoniacal liquor when subjected to evaporation deposited slight traces of iron, but as long as there was ammonia in excess  
it

it retained its blue colour, which, as soon as this free alkali was evaporated, was changed to a meadow green. The liquor constantly preserved this colour, which still acquired greater intensity until complete evaporation, and without forming any deposit, which proves that nickel existed in the combination in the state of a triple salt.

Fixed caustic alkalies occasion no precipitation in this liquor; but they make it resume its blue colour by decomposing the muriate of ammonia and laying bare its base, which then redissolves the nickel.

The hydro-sulphurets formed in it an abundant black deposit; and this was the method employed for obtaining the nickel separate. The hydro-sulphuret of this metal, calcined in a platina crucible, left a powder of a dark meadow-green colour, which had all the properties of oxide of nickel.

The iron contained in the stones which have fallen from the heavens is therefore combined with sulphur and nickel; whence it appears very probable that these two substances, which always present themselves in a quantity more or less considerable in the analysis of the whole stones, arise from a similar combination, which it is impossible, whatever care may be taken, to separate exactly from the earthy parts.

The presence of nickel and of sulphur in this iron explains why it is whiter, harder, and less ductile than common iron.

Though I did not ascertain very exactly the quantities of these substances in the iron, I think I may assert that they do not amount to more than five or six per cent. In regard to the pyrites disseminated here and there throughout these stones, it is formed, as Mr. Howard says, of iron, sulphur, and a small quantity of nickel; but I was not able to ascertain in what proportions these three substances are united, because I had not a sufficient quantity to subject them to an exact analysis.

It appears to result from all the information obtained, and from testimonies worthy of credit, 1st, That masses, sometimes of considerable size, have fallen to the surface of the earth: 2d, That these masses penetrated with fire move in the atmosphere like inflamed balls, which throw light and heat, and to a great distance: 3d, That they seem to have received a motion parallel to the horizon, though they really describe a curve: 4th, That they fall in a state of softness or fusion like paste, as is attested by their varnished surface, and the impressions formed on them by the bodies which they meet with: 5th, That some of them have fallen in England, Germany, Italy, France, and the East Indies: 6th, That all

these stones have a resemblance to each other by their physical characters and their chemical composition.

What are the causes which can produce stones of this kind, and communicate to them so rapid and so singular a motion? How comes it they are always penetrated with fire? These are questions for which at present it would be difficult to assign plausible reasons.

But whatever these causes may be, if multiple they must be of the same nature, since all the stones which have fallen in countries so different have in every respect a resemblance to each other.

Do they owe their origin to volcanoes? But where are these volcanoes? We are not yet acquainted with them; and stones similar to those in question have never been found among the productions of any of the known volcanoes. Is the atmosphere the medium in which they are formed? But how can we conceive that substances so heavy as earths and metals could exist in sufficient quantity, and remain long enough suspended in a fluid so light as air? If we suppose that these bodies existed in the atmosphere, whence did they originally come, and what means were so powerful as to unite them, and to form of them masses so heavy and so voluminous?

The opinion which makes them come from the moon, however extraordinary it may appear, is, perhaps, the least improbable; and if it be true, that no direct proofs can be given of this opinion, it is equally certain that no well-founded reasoning can be opposed to it.

The most prudent course to be pursued in this state of things is freely to acknowledge, that we are entirely unacquainted with the origin of these stones, and of the causes which produced them.

LXIII. *Conjectures on the Stones which have fallen from the Atmosphere.* By EUSEBIUS SALVERTE\*.

THE antients never entertained any doubt in regard to what they observed. As the natural sciences among them were only collections of facts, no theory compelled them to contradict their observations. We accuse them of often having seen without examining; but though we examine better ourselves, we are often obliged to abjure our scepticism, and at length to see what the antients saw before us.

\* From the *Annales de Chimie*, No. 133.

Among the phænomena to which this reflection may be applied, there is none more remarkable than the fall of stones, produced, according to the antients, by thunder. This is what they called thunder-bolts, an expression long ago banished to the domains of poetry, and which it would appear ought to acquire a place in the language of philosophy.

The antient historians all make frequent mention of the production of these stones. No doubt was entertained respecting them in the middle ages; but the difficulty of accounting for them induced us not only to suspend our belief until called forth by more regular observation, which was very prudent; but also, which was less reasonable, to carry with us in this research a predetermination to see nothing, or to deny what we had seen.

Notwithstanding this disposition, instances have been so multiplied before our eyes, and in so uniform a manner, that it becomes difficult not to admit the general fact, whatever opinion may be adopted in regard to the cause. Stones absolutely foreign to the soil where they were found, and all having the greatest resemblance to each other, have been collected, in Portugal in 1796, in Alsace in , in Yorkshire in 1796, at Sienna in 1794, at Benares in 1798, in Bohemia in 1753, near Paris in 1768. Several have been found still warm; and uniform tradition states, that they were seen to fall from the atmosphere in the time of lightning, and particularly during the bursting of luminous meteors, the production of which often accompanies storms. The papers of Mr. Howard and of Count Bournon contain very satisfactory details in regard to every thing observed in regard to the nature and fall of these stones.

The *Journal de Physique*, Brumaire, an xi, contains a memoir of C. Patrin, who, denying the celestial origin of these stones, supposes that they have only been uncovered and forced from the earth by the contact of thunder. But before this hypothesis can be established, would it not be necessary that in the places where these stones have been found, and in others, similar ones should have been previously discovered at the depth of a foot, or of some inches below the surface? For why should they show themselves at the surface of the earth only after thunder? Why should they constantly escape the plough and the spade of the farmer, the pick-axe of the ditcher, and the researches of the mineralogist?

To admit that these stones have been thrown up into the atmosphere by volcanoes, seems to be attended with the same difficulty; for all those analysed by Vauquelin contain a large proportion of iron and nickel, in the native state, mixed with

magnesia. But nothing is more rare in the volcanic productions, with which we are acquainted, than magnesia, if we except iron allayed with the nickel in the native state. Besides these stones have been collected at such a distance from all volcanoes, that the power which could so long support and convey through the air such enormous masses, would be still more inexplicable than their spontaneous production.

Chemists have proved, 1st, That these stones, collected in places and at times distant from each other, have a resemblance in regard to the substances of which they are composed, and differ only by the proportions of their elements: 2d, That they differ from all the other mineralogical compounds found at the surface of the globe. It is natural, therefore, to assign to them all an origin of the same kind, and very different from that by which the minerals hitherto observed have been produced.

Struck with these singular characters, C. Delaplace and Biot mentioned, during the discussion which took place in the National Institute in consequence of Vauquelin's analysis, a very bold opinion, but which is rendered probable by the celebrity and character of its authors. According to them; these stones may be the product of a volcanic eruption in the moon. Comparing the mass and density of the moon with that of the earth, and calculating the distance between our planet and its satellite, they have established that a volcanic eruption might project a body to such a distance from the moon as to be within the sphere of the earth's attraction. It is besides proved, by astronomical observation, that the atmosphere of the moon is exceedingly rare, and consequently could oppose very little resistance to the elevation of stones projected from the moon. To this may be added, that as the moon has scarcely any atmosphere, this circumstance may serve to explain why the metals contained in these stones are not in the state of oxide.

But the same observation which has discovered volcanoes in the moon, shews them in a state of luminous ignition, and hitherto luminous ignition has never been known to take place without oxygen. All the gases absorb caloric, and on becoming concrete, suffer it to escape. Oxygen alone possesses the property of absorbing light, and of emitting it when it combines with bodies during combustion. It thence follows, 1st, That metallic substances projected from the moon ought to be oxidated, at least in great part: 2d, That the observations which prove the great tenuity or little extent of the atmosphere of the moon, ought to induce us to believe

believe that the volcanoes which burn at its surface are in a state of tranquil inflammation, maintained by a very thin stratum of atmospheric air, very different from that of our volcanoes in a state of eruption; and, consequently, that they cannot be endowed with a very strong projectile force: for on the earth this force is produced by the expansibility of gas compressed by the weight of the atmosphere. But gases disengaged from the lunar volcanoes ought to expand without any obstacle, and without any explosion where there is scarcely any atmosphere. Besides, the existence of these gases is doubtful, since their production would soon have formed around the moon an atmosphere of greater density and extent, which is contrary to observation. This explanation therefore must be abandoned, or we should be forced to admit a series of phenomena, not only foreign but contrary to what we daily observe: a solution more simple, and deduced from facts, which daily take place before our eyes, will perhaps appear more proper for solving the problem. It is well known that there are no metals which may not be volatilized by heat: it is known also that hydrogen gas exercises its dissolving power on charcoal, iron, and the most fixed substances. If we suppose that, during the burning process of volcanoes, or the less perceptible but more assiduous process of the decomposition of organic bodies, iron and nickel are sublimated by heat and elevated with the hydrogen gas which dissolves them, this gaseous solution will soon reach the upper regions of the atmosphere. There, as I have shown, with some degree of probability, in my *Conjectures on the Diminution of the Waters, &c.* exists, and is continually renewed, a stratum of hydrogen produced by the continual decomposition of the water, and which causes the greater part of the phenomena that accompany thunder and the aurora borealis. During storms, that is to say, when the equilibrium is restored with an explosion between the electricity of the earth and that of the atmosphere, what must take place? The hydrogen inflames and exhibits some of those luminous meteors, the existence of which, according to constant tradition, ought, as appears, to precede the formation of stones. The gas in burning abandons the metal it has dissolved, and reduces that which was in the state of oxide. The strong heat produced at this moment fuses the metal, and molecular attraction collects it into masses of greater or less size, which, when they fall to the earth, retain for some time a portion of the caloric developed during their formation. The surface only, which, by traversing the atmosphere in a state of ignition, may have ab-

forbed oxygen, is slightly oxidated : the interior part is metal in a native state.

C. Patrin observes, in support of his hypothesis, that the discharge of an electric battery, on a fragment of the stones found at Benares, produced on it a black trace similar to the black vitrified crust with which they were covered. This fact indicates what takes place in the atmosphere, where these stones are formed amidst a very powerful electricity, which produces the vitrification and oxidation of their surface.

The same author says, with Mr. Howard, that, since no doubt is now entertained of thunder and the electric fluid being the same thing, the idea of a *thunder stone* is become ridiculous. Nothing could be more true, if thunder were never any thing more than an electric explosion : but the French chemists, and particularly Fourcroy, have established that hydrogen acts a considerable part in the phænomena by which thunder is accompanied. I am even of opinion that its existence ought to be oftener employed in explaining the varieties which continually render the general phænomenon complex.

The most remarkable and most common are perhaps those luminous meteors, the aspect of which is always brilliant and often alarming, and which uniform tradition, at periods and in places very distant from each other, assign as the cause of the stones which have fallen from the heavens : whether they seem to be confounded with the stars, and exhibit to the vulgar eye stars thrown from their usual course, or whether they accompany thunder, and increase or modify the action of its devouring flames, or whether, under the form of fire balls, they rapidly traverse the atmosphere, and approach near enough to us to make us experience a heat proportioned to the vivacity of their light, they must be considered as the effect of the inflammation of hydrogen gas, either pure or charged with substances in solution, which modify their appearance and products. On these principles we may point out the supposed causes of the difference which exists between stones that have fallen from the atmosphere at different periods. A very violent shock of electricity, or great heat, have produced those which are most vitrified : those less so have been formed by a weaker degree of electricity, and perhaps by spontaneous inflammation. Those which contain the largest quantity of metal in the native state, are the product of a greater proportion of hydrogen gas. It may be readily conceived, that when the hydrogen gas has been previously combined with a great deal

deal of oxygen; the inflammation must be instantaneous, and the products very much oxidated, while the strata of hydrogen, almost pure, burning only at the surface, form durable meteors, the continued inflammation of which gives rise to very different compounds.

It will perhaps be asked, how the other substances, such as flex and magnesia, are found in these stones combined with the metals? I might, in answer, mention the height to which earths reduced to an impalpable powder are volatilized by volcanoes, so as to be carried away by the winds. I might observe, also, that magnesia, soluble in the hydro-sulphurets, is soluble also, in all probability, in sulphurated hydrogen. But, as I have nothing as yet except conjectures to offer on this subject, I must here stop. I, however, flatter myself that I have done some service, by shewing that a phenomenon, observations of which had been rejected because thought impossible, has received, at least in part, a plausible and natural explanation perfectly rational; and that, when placed beyond a doubt, it will be arranged among those series of facts of which science is composed.

That I might not render the hypothesis complex, I have said nothing of the different gases, such as sulphurated hydrogen, carbonated hydrogen, and carbonous oxide, which must be continually conveyed into the regions of the atmosphere by the decomposition of bodies. The existence of the first of these gases explains, however, the pyritous state of the iron, and the presence of sulphur in some one of its parts.

LXIV. *Observations on the Decomposition of the Acetite of Lead, by Zinc in the metallic State. Read in the Society of Pharmacy by L. ANTHONY PLANCHE\*.*

IN consulting the different authors who have treated on the history of metals, and their chemical action on different bodies, we cannot help considering zinc as exceedingly valuable, both on account of its utility in the arts, and the essential services which it daily renders to chemistry.

C. Fourcroy, who in his *Système des Connoissances Chimiques*, has collected the most numerous and best attested facts of this science, has described all its properties with that clearness and method which are peculiar to him.

\* From the *Journal de Chimie*, No. 133.

It is therefore not so much a new fact that I here present as a development of one already known, which, as it forms part of a general theory, deserves to be particularly noticed as one of the curious phænomena in chemistry. But to proceed in order, I think it necessary to refer to the work of that celebrated chemist already mentioned, where it is said, in the sixth section on metals, under the head Zinc, that

“Zinc, by its strong attraction for oxygen, decomposes the greater number of salts and of metallic solutions; and that it precipitates from them the metals under the metallic form by completely unburning them, or under that of oxides less oxidated than they were before.”

This definition is no doubt just, and seems to be perfectly complete. It is susceptible, however, of the greatest development; and particularly in regard to the different phænomena exhibited by zinc in contact with acetite of lead. It is the union of these phænomena which forms the principal subject of the present observation, or rather the result of the ingenious experiment of Dr. Black, which was communicated to me a few years ago, and to which I have made some additions.

Dr. Black put into a glass jar, containing about four and a half pints, twelve or fifteen decagrammes of crystallized acetite of lead. The jar was filled with water, and the mixture being stirred, it was left at rest for fifteen or twenty minutes, or until the greater part of the superabundant salt, after the saturation of the liquor, was spontaneously precipitated. A piece of zinc of an indeterminate weight was then suspended in the slightly turbid liquor by means of a hemp or silk thread fixed to the cover of the jar, and the vessel was deposited in a place where it was not exposed to be shaken. At the end of from twenty to twenty-five days decomposition was completely effected; the zinc, suspended in the middle of the liquor, became diaphanous, was covered by a kind of very brilliant metallic vegetation, which was often continued to the bottom of the vessel, and which Dr. Black called the tree of Saturn.

Having had occasion to repeat this experiment a great number of times, it conducted me to several observations, in consequence of which I resolved to make some changes in it, which, as they do not alter in any manner the chemical nature of the operation, contribute in a singular manner to the beauty of the result. These changes chiefly relate to the hemp and silk threads, which, being easily destroyed by remaining in the liquor, must necessarily suffer the zinc to fall, and along with it the new metal which covers them. I

therefore substituted for these threads a piece of brass wire which appeared to me to unite all the necessary conditions, both on account of its strength, and the property it has as containing zinc to exercise its attraction on the acetite of lead. A second inconvenience arises in strictly following the process above described. It often happens, when the size of the piece of zinc is too large, or when the vessel is not exactly of the proper form, that the decomposition takes place with great rapidity; each molecule of the new metal proceeds immediately to the zinc, adheres to it in a confused manner, and at the end of two or three days, and sometimes of twenty-four hours, the crystallized portion suddenly detaches itself and is precipitated to the bottom of the vessel to give place to a new crystallization. The process is then divided into several periods, which appears to be a matter of indifference in a chemical point of view, but greatly lessens the interest excited by this curious experiment.

I obviated this inconvenience by fixing, in a perpendicular direction, to the lower part of the piece of zinc, the same brass wires twisted into a spiral form, and disposed in such a manner as to be about three centimetres distant from the bottom of the vessel.

By adopting this modification, as each molecule of the zinc formed a portion of the brass, the surface of which was in contact with the saturation, it became covered with a great number of small, brilliant, metallic laminæ, disposed at first alternately and horizontally, and then crossing each other in every direction\*.

When it is required that the brass, though covered with metal, should retain the forms it has first acquired, it will be proper to diminish the quantity of the acetite of lead.

About six months ago, I repeated this experiment with common and with distilled water, in order that I might examine the products. The result of this examination is as follows:

Having selected two glass jars of equal capacity, I introduced into each four decagrammes of very pure crystallized acetite of lead.

The two vessels having been marked No. I. and II., I poured into the first 54 decagrammes of common water, and as much distilled water into the second. These quantities

\* The configuration of these laminæ is subject to an infinite variety, which seems to depend, 1st, On the form and different dimensions of the vessel in which the experiment is performed: 2d, And perhaps also on regular quantities of the zinc and acetite of lead employed. I have seen the laminæ sometimes circular, sometimes hexagonal, and sometimes also affecting the figure of a leaf of fern or of oak.

were sufficient to fill each jar to the commencement of the neck, the height of which was about three centimetres. I then fixed to two pieces of cork destined to close the mouths of the jars a bit of zinc weighing 22.9456 grammes (six gros) by means of a piece of brass wire exceedingly fine, but of such a length as to allow the metal to be immersed only some millimetres in the solution.

I then adjusted, and luted to the aperture of each vessel, a bent tube, which proceeded under the receiver of a pneumatic tub. These two experiments, begun on the 29th of Thermidor, year 9, the centigrade thermometer being at 14° above zero, exhibited the following phænomena.

Six hours after the apparatus was arranged, the jar No. I. was still somewhat turbid, while that of No. II. was transparent, and the whole superabundant salt, after the saturation of the liquor, was entirely precipitated. At this period the zinc in the second jar was covered with small metallic scales, which could not be perceived in the first.

Next day both liquors were perfectly transparent, and the decomposition seemed to have made equal progress in both vessels. I only remarked, that the interior sides of the jar No. I. were lined with a whitish saline stratum, which extended from the middle of the vessel to the bottom. The small scales adhering to the sides and bottom of the piece of zinc, by increasing in volume, had assumed the form of a leaf of fern in the jar No. I., and was exceedingly delicate in No. II.

I observed also at the upper surface of the bit of zinc a kind of metallic moss\*, having that livid aspect which characterizes plates of lead exposed for some time to the contact of the air. The saline stratum of No. I. daily decreased to the eighth, when it entirely disappeared. In the course of these two experiments there was disengaged a very small portion of elastic fluid, which when examined was found to be atmospheric air. The quantity of this air appeared to be more considerable in the apparatus marked No. I.; and this appeared to me to arise from the common water employed, which always contains more or less of it. The remainder of the operation exhibited nothing remarkable but an increase in the volume of the reduced metal until the period of the

\* This metallic moss in a tree of Saturn, prepared a year before, was found to be covered by a very fine red powder, which occupied both the surface of the zinc and that of a portion of the undecomposed acetite of lead which covered the horizontal bottom of the glass vessel. I consider this red powder as produced by carburet of iron, from which zinc is never free, and which during the operation has been carried to the state of carbonate.

decomposition, which took place in the experiment with distilled water on the 16th of Fructidor, year 9, and in that with common water on the 24th of the same month. During the whole time the temperature varied only one or two degrees.

As the results appeared to be the same in regard to the rest of the operation, I shall proceed to examine the experiment made with distilled water.

1st, The piece of zinc, weighing 22.9456 grammes before its immersion in the liquor, was completely freed from all the metal with which it was covered. In this state it weighed no more than 15.9345 grammes (4 gros 12 grains). Its surface was covered with a gray pulverulent stratum, the greater part of which still retained its metallic brilliancy, while the other seemed to have undergone a commencement of oxidation. When entirely freed from this stratum, which could be easily detached with the finger, and which appeared to me to be nothing but zinc, having its *moleculæ* in part separated, it was reduced to the weight of 13.380 grammes (3 gros 37 grains).

2d, The portion of the new metallic alloy, zinc and lead \*, arising from the decomposition of the acetite, was washed several times in distilled water. Being subjected to the action of a good press, and then to gradual percussion on a piece of steel, it possessed greater ductility than lead alone. When dried in close vessels, and then exposed to sudden heat in an iron spoon, a portion of it was speedily converted into a greenish yellow oxide. The other portion, reduced without any addition, furnished a small metallic button of the weight of 9.0183 grammes (2 gros 38 grains), and containing zinc, but much less weight for weight than was contained in the above matter before it was heated.

I must observe, that, notwithstanding all the precautions which I took to separate, either by the help of a press and unsized paper, or by desiccation, all the water interposed between the *moleculæ* of the metal, I was not able to succeed. It appears probable, that under this circumstance the speedy oxidation of the new metallic alloy was owing, 1st, To the decomposition of the last *moleculæ* of water, which could not be entirely expelled: 2d, To the presence of the atmospheric air of the vessels: 3d, To the contact and decomposition of the same fluid during the fusion of the metal.

\* The formation of this metallic alloy agrees with the experiments of Vauquelin, from which it results, that in several cases, when a metal is precipitated from its solution by another, the precipitated metal partakes in some degree of the precipitant. This phenomenon takes place in particular during the precipitation of the white metals by zinc.

*Examination*

*Examination of the Liquor with some Reagents.*

1st, The liquor taken from the jar was colourless and perfectly transparent.

2d, It gave a strong green tint to blue vegetable colours.

3d, When tried with very pure sulphuric acid it formed no precipitate, which convinced me that it held in solution no lead.

4th, A portion properly evaporated furnished small crystals of acetite of zinc in argenteous leaves, which affected no regular form.

5th, Ammonia produced in it a white flaky precipitate, not very abundant. At the moment even of contact there is emitted a strong odour of flour paste, which is speedily dissipated by agitation.

6th, This precipitate at the end of some hours had acquired a semi-gelatinous consistence, and the addition of a new quantity of alkali redissolved it in part.

7th, The carbonates of potash, soda, and ammonia, formed in it white precipitates more or less abundant.

8th, Lime water recently prepared precipitated from it the zinc in the state of a white oxide, paler and more homogeneous than the preceding.

9th, The precipitate obtained by carbonate of potash, washed several times with distilled water, and carefully dried, was exceedingly white and light.

10th, Being brought into contact with concentrated sulphurous acid, it dissolved in it completely, with a disengagement of carbonic acid, and a total disappearance of the sulphurous odour; which seems to show a very great relation between the new salt resulting from this combination and the sulphate of zinc, for the discovery of which we are indebted to Vauquelin and Fourcroy.

## RECAPITULATION.

It results from these facts, 1st, That the decomposition of the acetite of lead by zinc exhibits several phenomena not observed in that of other salts and metallic solutions by the same metal.

2d, That distilled water seems to favour this decomposition, as it is effected in that fluid a third sooner than in common water.

3d, That common water may, however, be preferable in experiments of mere curiosity, as it checks the decomposing action of the zinc, and disposes the metallic molecularæ to assume that arrangement which is peculiar to them; a phenomenon which agrees with the laws of crystallization.

I shall terminate these observations by proposing to the  
College

College of Pharmacy to adopt among its experiments, which form a part of its annual public course of chemistry, the tree of Saturn, as proper for proving in a precise manner the strong attraction of zinc for oxygen. This experiment, which is not very expensive, always succeeds, and deserves a place in mineralogical cabinets along with the tree of Diana.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

THE meetings of the 12th and 19th of May were occupied by the reading of a paper, by Mr. Chenevix, on a metallic substance, of which we have spoken in our last Number. It was announced to the public by its author as a new simple metal *sui generis*, under the title of *Palladium*, or *new Silver*.

Mr. Chenevix made a number of experiments on this substance, and found that, in most respects, it was really different from all the known metallic bodies. In a few, however, it bore some resemblance to platina. But it was not possible to conceive that platina could be reduced to the specific gravity of 11.8; could be fusible at so moderate a heat; could combine with sulphur; could be dissolved by nitric acid, and be precipitated by green sulphate of iron. These are the properties mentioned in the printed notice we received last month, and which we inserted in our Journal. Mr. Chenevix found them to be truly stated.

Reflecting, however, upon the extraordinary anomalies that happen when substances are united, it occurred to him that some metal might so far influence platina as to promote its precipitation by green sulphate of iron, and be itself drawn down along with it. Mercury was the first that presented itself to his mind, as the most easily reduced. He poured a solution of green sulphate of iron into a solution of platina: no precipitate. Into a solution of muriate of mercury: no precipitate. He mixed the two solutions, and a precipitate, exactly resembling that produced in palladium, immediately ensued. He reduced this precipitate; and, after several trials, at length succeeded in obtaining a substance exactly similar to palladium. From all his experiments, which are much too numerous for this extract, Mr. Chenevix does not hesitate to pronounce palladium to be a scandalous imposition, and an attempt to defraud the public as well as to disgrace science.

From synthesis and analysis it appears that palladium of the specific gravity of 11.5 contains about one-third of mercury; and when of the specific gravity of 12.5, about one-fourth.

fourth. As the quantity of this metal diminishes, the specific gravity increases; the ductility is less; and after 12.5 it begins to be no longer acted upon by nitric acid.

Mr. Chenevix gives many methods for preparing a substance similar to palladium; but the following is the least liable to fail:—Take a certain quantity of platina, reduced from the triple ammoniacal salt, and dissolve it in nitro-muriatic acid: add something more than twice the weight of the platina of red oxide of mercury, and take particular care to saturate all excess of acid: then pour it into a solution, also saturated, of green sulphate of iron; heat the mixture, and a black precipitate will be formed after some time. This precipitate weighs rather more than two and a half the original quantity of the platina; but, exposed to a red heat, a part of the mercury is volatilized. Fused at a stronger heat, it yields a metallic button, which in different experiments gives a different specific gravity. This experiment is of a very delicate nature, and does not always succeed; and Mr. Chenevix recommends perseverance and repeated trials to all who would form palladium. It is not often to be obtained of the exact specific gravity announced by the author of the fraudulent advertisement; and the specific gravity is not constantly the same in all his specimens. The difficulty is, to unite a sufficiently large portion of mercury with the platina to produce perfect palladium. It may be obtained by this method as low as 10.5, sometimes as high as 15. Mr. Chenevix mentions some methods of combining platina and mercury in the dry way, such as count Mushin Pufkin's amalgam fusing with cinna<sup>b</sup>ar, lime, and borax, &c.

He then relates some experiments upon platina; upon its oxides, which are two, viz. one of 7 the other of 13 per cent. of oxygen; upon the affinities of the oxide for the acid and aromatic salts formed by them. He gives numerous examples of the affinities of metals for each other; and of anomalous precipitations, when two or more metals are present in the same solution. He says, that the fixation of mercury by platina, together with other facts related in this paper, will tend to make us particularly cautious in pronouncing upon discoveries of supposed new metals, and promote scepticism even upon our present knowledge. He adduces instances of the difference that exists between a solution of platina and a solution of mercury merely mixed, and a solution of these metals after they have been united. In the former case they are easily separated; in the latter there is no method of completely disuniting them. He concludes with a curious statement of the approximation of several metals and of several earths to each other in their chemical properties, which fol-

low the direct order of their tendency to enter into saline combination.

The whole of the facts related in this paper are of the highest interest to the science, as they prove to what a degree the properties of simple bodies, hitherto thought to be uncombinable, may be altered when they are united; and show the difficulties that attend our pronouncing as to the simplicity or the composition of any substance, whatever be the character that distinguish it in chemical experiments.

ROYAL SOCIETY OF GÖTTINGEN.

The Mathematical Class of this Society has proposed the following prize question, the prize for which is to be adjudged in the month of November 1803:

As it is of great importance in pyrometric disquisitions as well as in the application of them, and also in making researches in regard to the nature of light and heat, to ascertain the various degrees of heat which different substances, sooner or later, acquire when exposed to the solar rays; and, as little certain information has hitherto been acquired on this subject, the society recommends it to the care and attention of philosophers.

1st, To investigate by nice experiments, and by calculation founded on them, in what manner bodies of different substances, but of the same figure and volume, (globes of an inch diameter will, perhaps, be best for this purpose,) are heated by the solar rays under the same state of the atmosphere, the same intensity of light, and the same initial temperament during each minute of observation, &c.

2d, To determine, either by direct observation, which is chiefly desired, or from the observed law of the increasing heat, to what degree of temperament any body taken at the end of the experiment, that is, when the increase of the heat ceases, would have attained.

The Historical Class has proposed the following question, the prize for which is to be adjudged in the month of November 1804:

As many observations have been carefully made by the old and by modern philosophers, particularly since the 16th century, in regard to meteors; as various ingenious opinions have been given respecting their origin and nature, and the laws to which they are subject; and as many things perhaps occur in these which might be of utility to improve meteorology, or which at least may be worthy of further examination, the society requires a continued accurate history of meteorology from the first attempts of the Greeks and Romans in this kind of study down to the present period.

The society, however, by no means desires that the competitors

petitors will include in it any of those opinions of the antient or modern writers which, being founded on superstition or ignorance, would excite only disgust. It rather wishes that they would select, explain, and examine the opinions of Aristotle, Theophrastus, Pliny, Seneca and Ptolemy, among the antients, and those of the philosophers of the 16th and 18th centuries, particularly Telesius, Patricius, Bruno, lord Bacon, Kepler, Gassendi, and Descartes, a knowledge of which may be of use to meteorology at present: it requires also, that the manner in which the observations were made, and the instruments employed, may be briefly described, marking at the same time the climate. An inquiry may also be made respecting the origin of that opinion, which became so prevalent, in regard to the influence of the planets on meteors: and, in the last place, the author may briefly and generally determine how far we can approach nearer to the truth by improving physical astronomy and meteorology.

The prize for each of these questions is fifty ducats, and the memoirs must be transmitted to the society before the month of September each year.

#### GALVANIC SOCIETY, PARIS.

Galvanic facts, in proportion as they are accumulated, afford a hope of the happiest results being soon obtained from the application of this stimulus. Those which we are here about to detail relate partly to theory and partly to the medical employment of this fluid.

In a late sitting of the Galvanic Society, C. Gautherot mentioned an observation made by him anterior to that period, which was both curious in itself, and of importance, on account of the inductions that may be drawn from it. As it is necessary to be known before a proper opinion can be formed of those by which it was accompanied, we think it proper to lay before the reader the two following experiments:

##### *Experiment I.*

The person who performs this experiment places in his mouth the upper ends of two wires of platina, or of any other metal not oxidable, and immediately brings the other two ends into contact with the two extremities of a weak Galvanic apparatus, to ascertain the degree of its influence. When this arrangement has been made, the person will experience an effect more or less remarkable, according as the tongue is more or less exercised in perceiving the peculiar flavour of the Galvanic fluid. But if he places these two ends of the platina wires one upon the other without deranging those placed in the mouth, he will again experience, though in a weaker degree, the sensation of the flavour.

##### *Experiment*

## Experiment II.

If the Galvanic apparatus has produced only a *minimum* of effect, which must be the case, 1st, When the apparatus consists of only one story; 2d, When the substances which compose this story are by their nature not much calculated for developing the Galvanic effect, or when they can produce it only in the weakest degree; it may happen under both these circumstances that the organ of taste will not be affected by any flavour. But in this case it will be sufficient to let the two lower ends of the platina wires rest for a moment on the two extremities of the apparatus, and then to apply these two wires to each other without deranging their extremities, which are placed in the mouth. This new contact will necessarily produce a more intense effect than the former, and if it be required to increase it still further, it may be done by applying alternately the lower ends of the platina wires, first to the two extremities of the battery, and then to each other. These repeated contacts will develop the peculiar flavour of the fluid, which is the certain sign of its influence.

This ingenious process, then, puts the tongue, that disposable instrument, in possession of a new *galvanoscope*, which, as appears to us, will be to Galvanism what Volta's condenser is to electricity, since by its means the slightest shades of the Galvanic effect may be detected.

C. Nauche, president of the society, in the same sitting communicated some facts in regard to the medical application of Galvanism in cases of hemiplegia. It results from them, that the means of giving a strong stimulus to the parts affected, such as the hand or leg for example, is, to make one of the extremities of the pile to communicate with the former, and the other extremity with the spinal apophyses of the sixth and seventh cervical vertebræ. If the leg be affected, the foot and twelfth dorsal vertebra must be made to communicate with the two extremities of the pile. The vital action will then seem to be conveyed as if by undulation to all the muscular organs in order to restore motion.

The president observed also, that in cases of great extremity, where it is necessary to increase in an extraordinary degree the excitement of the organic forces, one extremity of the pile must be brought into communication with the summit of the vertebral column, and the other with the spinal apophyses of the first lumbar vertebræ. The patient subjected to this experiment will perceive flashes, certain flavours and sensations, more or less painful, in the stomach, in the intestinal canal, and in the viscera of the breast and the abdomen. The muscles of the trunk and those of the extremities will be violently contracted.

A fact no less important, and which he confirmed also, is, that the application of Galvanism increases in a particular manner the action of the parotid, the lachrymal, and maxillary glands, that of the kidneys and of the whole lymphatic system. To produce this action the apparatus must be directed, not to the glandulous organs, but as much as possible to the origin of the nervous trunks which distribute themselves thither.

It may be readily conceived what advantage skilful anatomists may derive from an application of Galvanism to the different systems of the animal economy, especially if to anatomical knowledge they unite a thorough acquaintance with the theory of rational medicine.

TOURLET.

## LXVI. *Intelligence and Miscellaneous Articles.*

### MINING.

THE mines of Cornwall and its neighbourhood now yield so large a part of the metals so valuable to the arts, copper and tin, of all that the world produces of these substances, that the present state, and probable future one of productiveness, is highly interesting to those connected with the useful manufactures.

Tin, though the oldest object of the miner's search in Cornwall, now employs by far the lesser number of hands and amount of capital; the quantity raised of course is much diminished. Generally speaking, the tin mines have not been found to hold their richness to so great a depth beneath the surface as copper seems to do; consequently many, which were highly productive mines of this metal, now are stopped, or are worked to little advantage.

Polgooth, the greatest tin mine now in the world, though it produces large quantities of ore, yet it is attended with so great an expense from its depth, that it is understood to pay the adventurers now but very little profit. A report of cobalt being found in this mine has been heard, but we do not know whether it is entitled to credit; we wish it may, but, as far as we have had opportunities of judging of the minerals produced in this district, it does not appear to us probable that these scarcer ones will be found in quantities sufficient to repay any great expense in searching for them. The tin mines in the western part of the county, between Penzance and the Land's End, are doing well for their proprietors in general; and though they have for a few years produced but little, they now are throwing up good quantities of ore.

The

The quality of the metal lately brought to market has been found, we understand, much inferior to what it ought to be: this may be accounted for in the following way:—Grain tin particularly has been greatly in demand; in order to get more of the metal under this form the smelters have of late applied ores to produce it from, which are generally understood not to be sufficiently free from other metals for this purpose, and thus the grain tin, though increased in quantity, has been less pure: at the same time the common tin, being robbed of the better part of the ores from which that has generally been smelted, has suffered a like degradation in quality. It is to be hoped that the smelters will see very soon their true interest in this respect, and that the risk of injuring the trade may no longer be continued for the prospect of temporary gain.

The quantity of copper produced has not much fluctuated of late, though on the whole it may be expected, unless unlooked-for discoveries be made, to lessen; as several large mines have already stopped, the produce not being sufficient to counterbalance the enormous outgoings their great depth occasioned. Some other mines that have for many years, and even of late, produced almost as much as any in this part of the world, are expected soon to be abandoned, partly for the reason above stated, and partly because they have failed in quantity of ore in the lode or vein. On the other hand, some mines have bettered in their prospects: Dolevath, after having been brought into work at an enormous expense, has afforded a course of copper of great magnitude, and though the ore is not rich in proportion of metal, yet its quantity is large enough to make this discovery highly valuable. Wheal Towan, in St. Agnes, continues its productive state; and Wheal Crowndale, near Tavistock in Devon, is yielding a great deal of copper ore from a small depth under the surface.

Like tin, it has been found that copper is of late inferior in purity to what it was formerly, at least it is asserted that the sheathing of ships would a few years since remain in use a much longer time than any that can now be got will do. This probably arises in both metals from the same cause, from the greatness of the demand, which tempts the smelter to let improper alloys remain in a certain degree in his metal; but it well becomes them to reflect, whether this may not sooner than they expect be the means of destroying the demand all together, by driving the consumers to some substitute which may answer their purpose better. F.P.M.S.

## ANTIQUITIES.

M. Grotefend has published an addition to his explanation

of the Persepolitan Inscriptions. The author thought it necessary to prefix to this memoir an explanation of other larger inscriptions, in order that an opinion may be easier formed of it. In the first place, the alphabet of the wedge-form writing of the first sort has two fundamental strokes, which may be called the bow and the arrow, or rather the chisel and the rule. It appears to have been invented chiefly for lapidary inscriptions. It has no round strokes, and is not only very convenient for that purpose, but very beautiful, on account of its simple and regular composition. The author mentions as particular properties of this alphabet: 1st, The principal strokes of the letters are perpendicular; the horizontal are the accessory strokes. The latter are always on one side, or over the former; only in the letter *g*, there are at the top two wedges which cross each other. 2d, The points of the arrows, or wedges, are always turned to the bottom or to the right, when the characters are written proceeding from the left. This position is natural. 3d, Each letter has no more than three wedges, or two angles, as principal strokes, and at most three accessory strokes; however, when an angle forms the principal stroke, there is at times an accessory stroke on the other side. 4th, The angles and principal wedges are all of an equal height, only that in two letters, *m* and *o*, the middle wedge is shorter, in order to prevent their being confounded with the *t* and the *a*, which are similar. In regard to the transverse, or accessory strokes, one of them is often shortened; or one stroke, which ought to be at the top, is often placed at the side for want of room. 5th, In letters which have only one principal stroke, the transverse strokes are always on one side; where there are two principal strokes, the transverse strokes are in general at the top; where there are three, these accessory strokes are also on the side. 6th, When several transverse strokes are over one or two principal strokes, the latter are shortened that the letter may not pass the line. The alphabet itself is represented under a double point of view; first, according to the composition of the strokes, where they begin by the simplest signs, the wedge with one or two accessory strokes; and then according to the order of the letters of the Zendic alphabet. In the last table of Anquetil the author observed letters which he thinks defective, according to a comparison with the plates of Bruyn and Niebuhr. In the last place, the author remarks, that this wedge alphabet has no resemblance to any of the known alphabets; and that it would be vain to attempt to determine the meaning of the single letters according to their resemblance with the Zendic or other letters: the author, however, finds that the letters which resemble each other in the Pehlvi writing at

Nakschi

Nakschi Rustan, resemble each other also in the wedge writing.

Secondly, What the author observes in regard to the Zendic language may be reduced to the following objects:— 1st, The Zendic among the Persian dialects is what the Sanscrit is among the dialects of the Hindus, and has a great resemblance to that language. Neither of them is formed, but they are abundant in words; both have a great many elevated vowels, and, on the other hand, have very harsh combinations of consonants. The two languages have also several things common in the radical words and in the flexions, and are very different from other languages. 2d, The Zendic has a great variety of grammatical forms, and a construction without rules; like the Greek and the German it abounds in compound words. The most important thing in this section is an example of the declension of the noun taken from inscriptions, and which in several parts deviates from the declension of Anquetil. The singular, for example, has in the genitive and dative the termination *ābē*, (Anquetil has *tschao* or *ao*;) the accusative has *o*, (Anquetil has *m*,) *e*, and *etschao*, (instead of the *bio* and *bietscha* in Anquetil.) These differences would be striking did we not reflect, that the two sorts of declension are taken from partial examples, and that we are less acquainted with the Zendic grammar than with its dictionary.

#### ASTRONOMY.

Table of the geocentric motion of the two new planets for June 1803.

Geocentric Motion of Pallas.					Geocentric Motion of Ceres Ferdinanda.				
		Right Ascension.		Declin. North.			Right Ascension.		Declin. South.
June 2	19 <sup>h</sup>	7 <sup>m</sup>	13 <sup>s</sup>	26° 2'	18 <sup>h</sup>	45 <sup>m</sup>	0 <sup>s</sup>	22° 29'	
5	19	5	27	26 16	18	43	2	22 43	
7	19	3	30	26 31	18	40	57	22 56	
11	19	1	23	26 45	18	38	44	23 5	
14	18	59	3	27 0	18	36	27	23 12	
17	18	56	35	27 14	18	34	6	23 16	
20	18	53	56	27 29	18	31	38	23 19	
23	18	51	14	27 43	18	29	9	23 18	
26	18	48	22	27 56	18	26	37	23 16	
29	18	45	29	28 9	18	24	6	23 9	

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END OF THE FIFTEENTH VOLUME.

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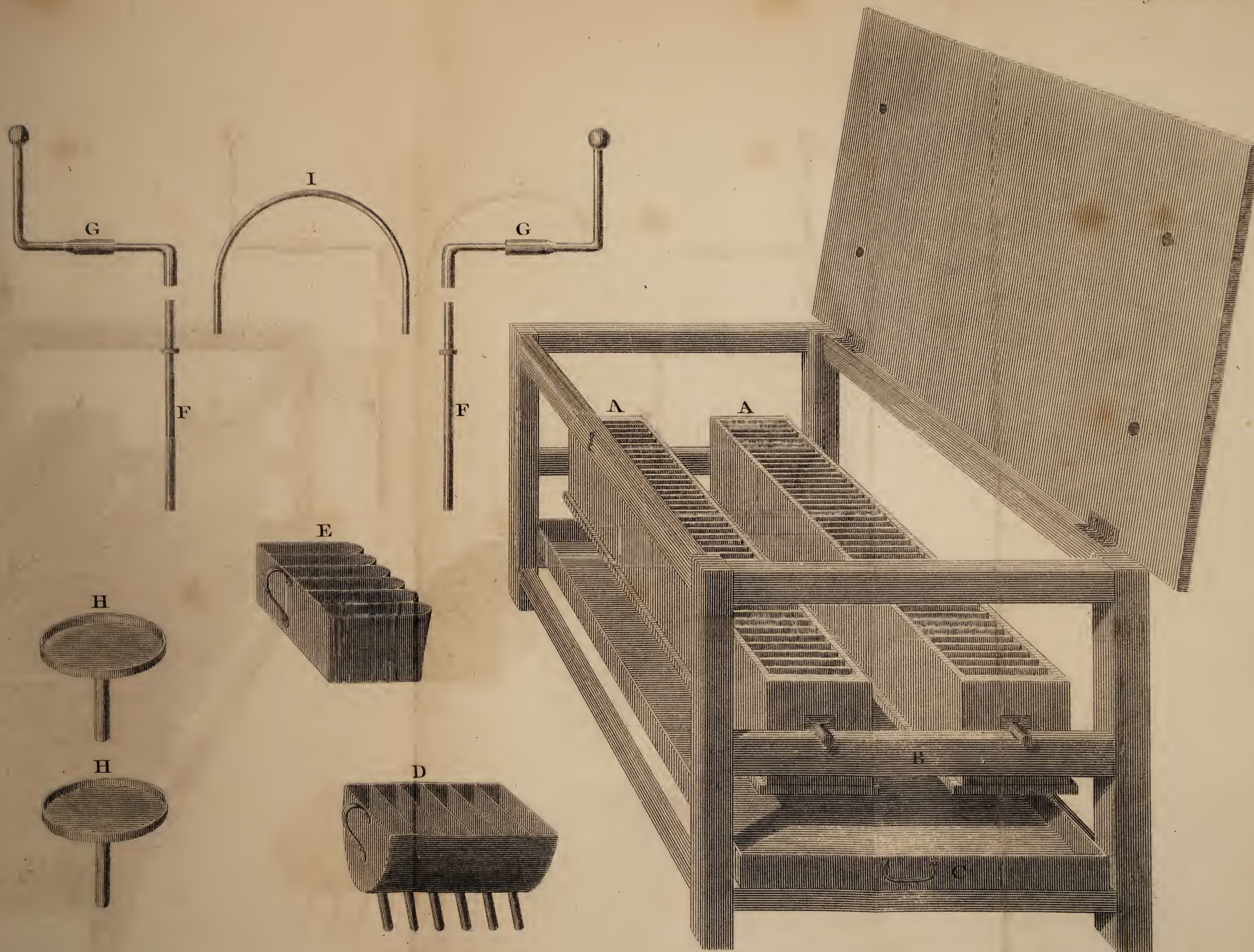




Fig. 2.



Fig. 1.



Fig. 3.

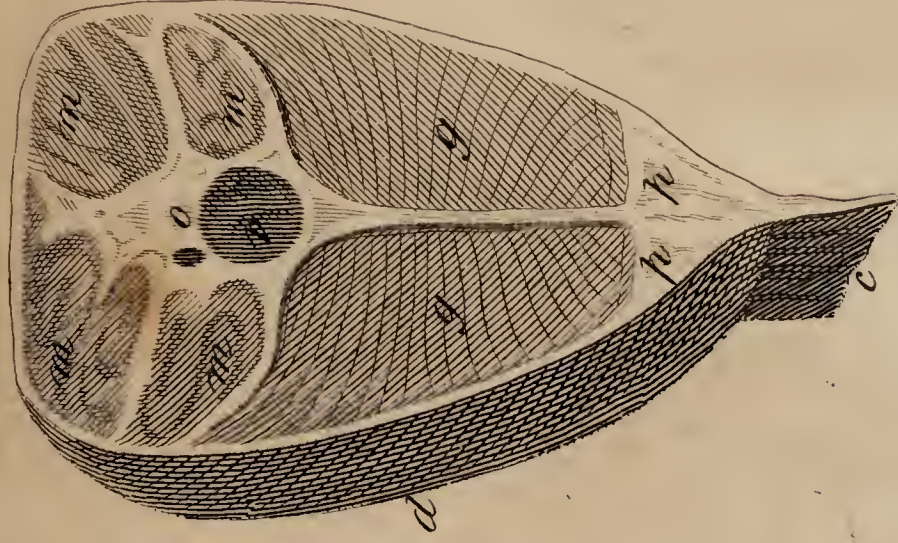
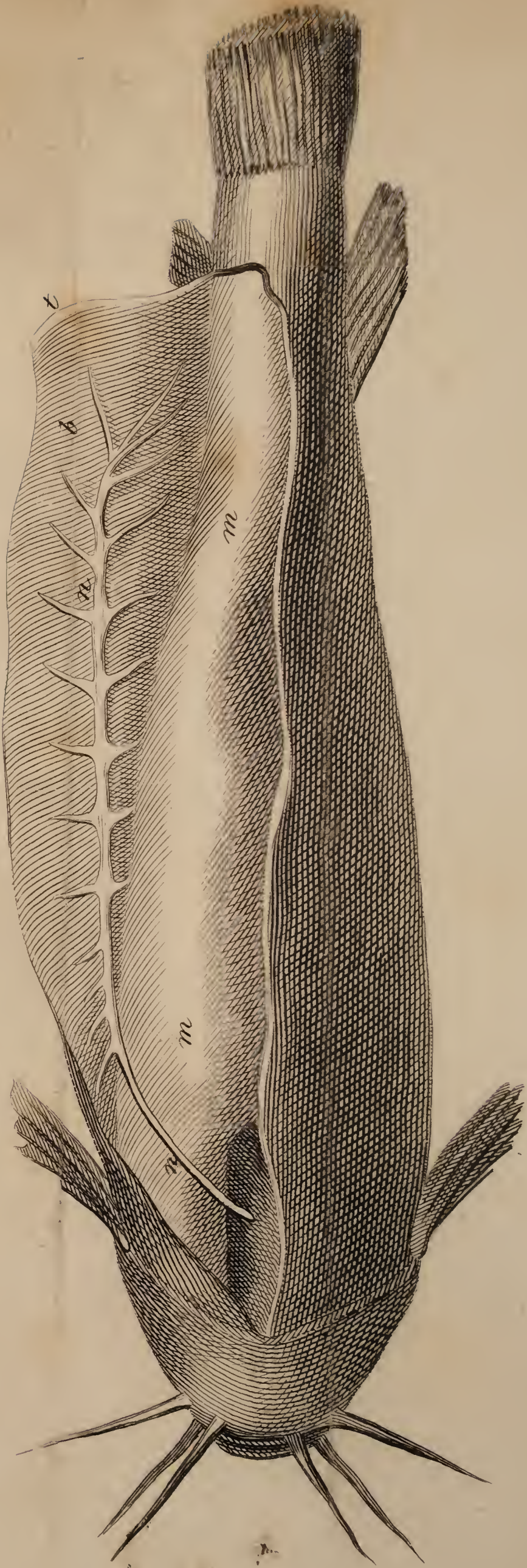
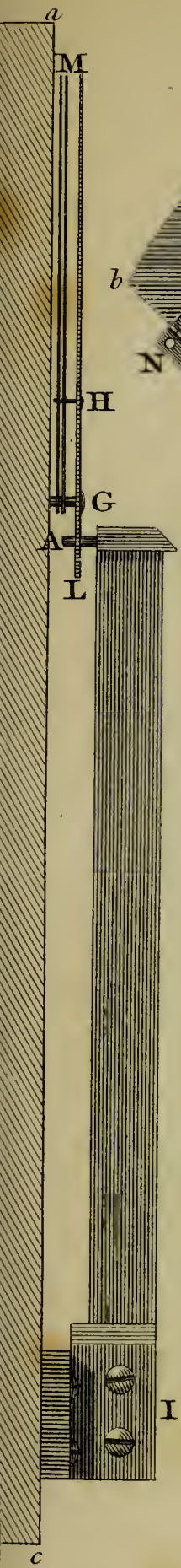


Fig. 4.





*Fig. 2.*



*Fig. 1.*

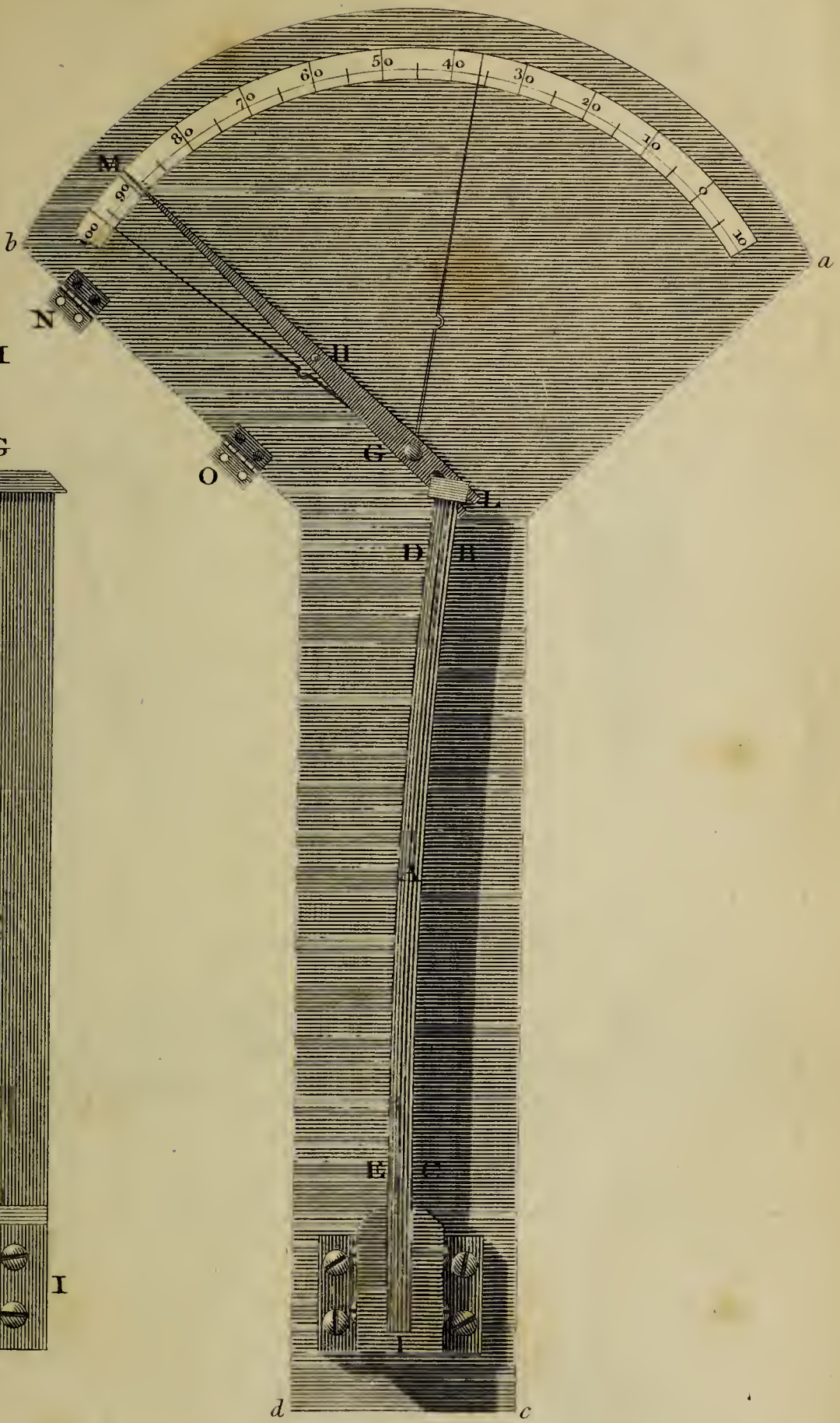




FIG. 1.

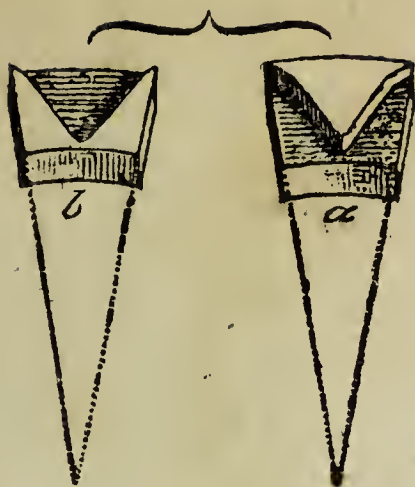


FIG. 2.



FIG. 3.



FIG. 4.



FIG. 5.

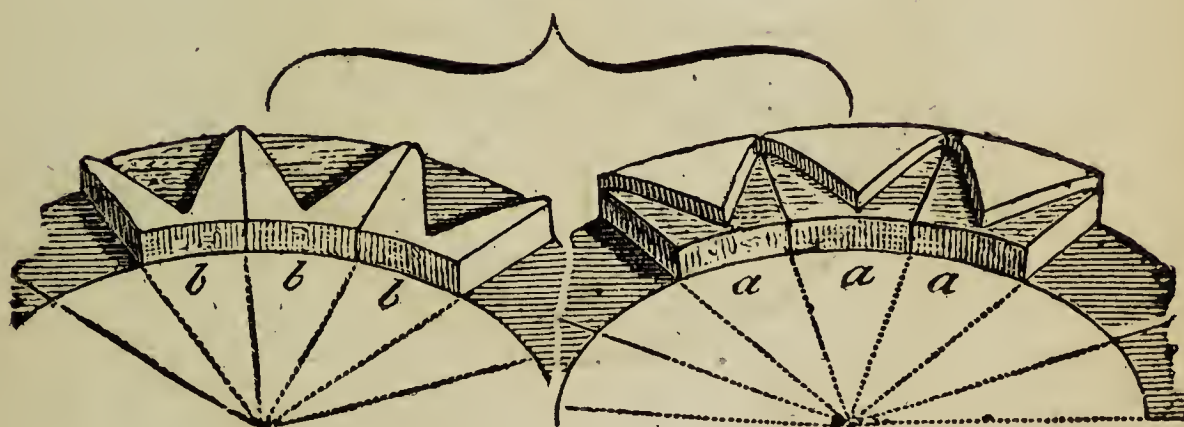


FIG. 6.

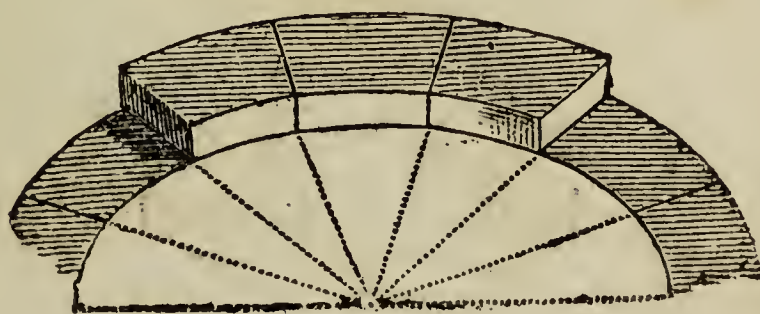


FIG. 7.

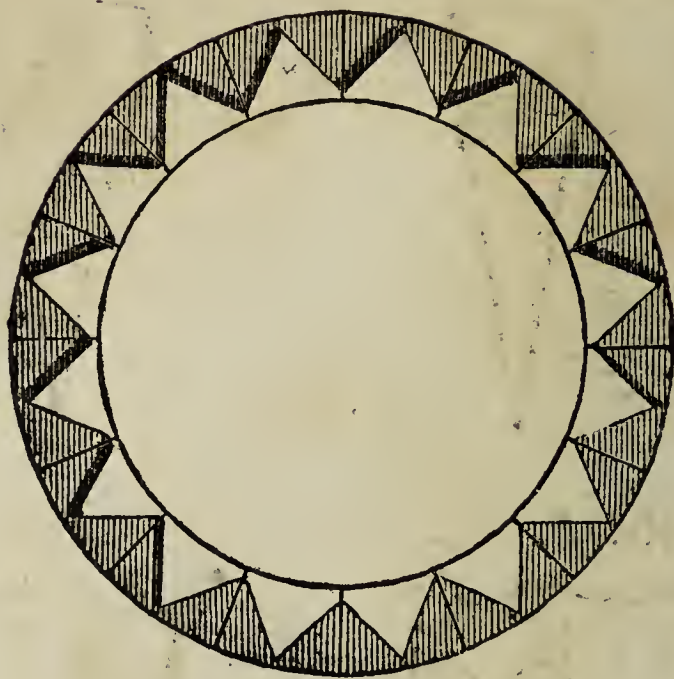


FIG. 8.

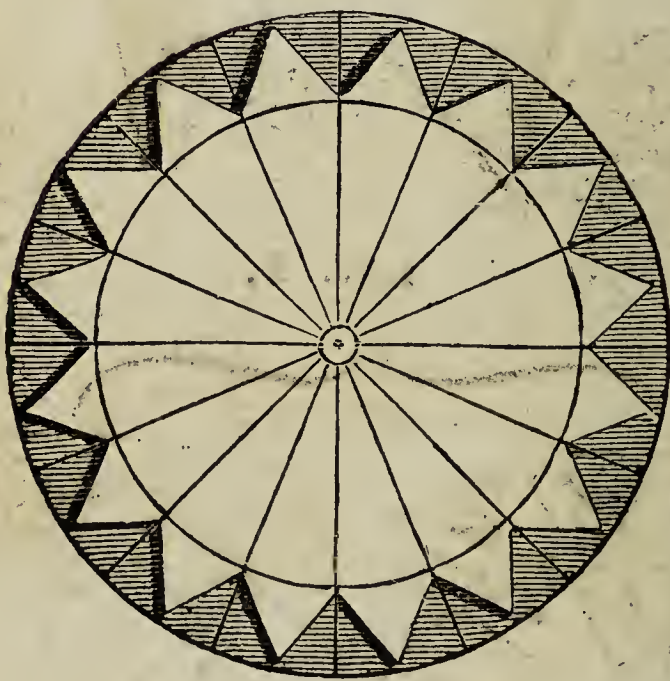
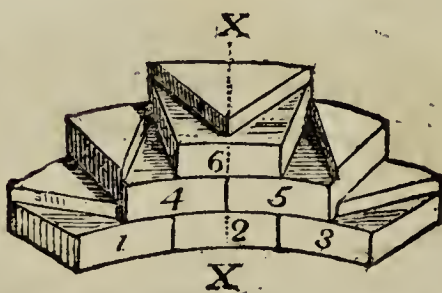


FIG. 9.



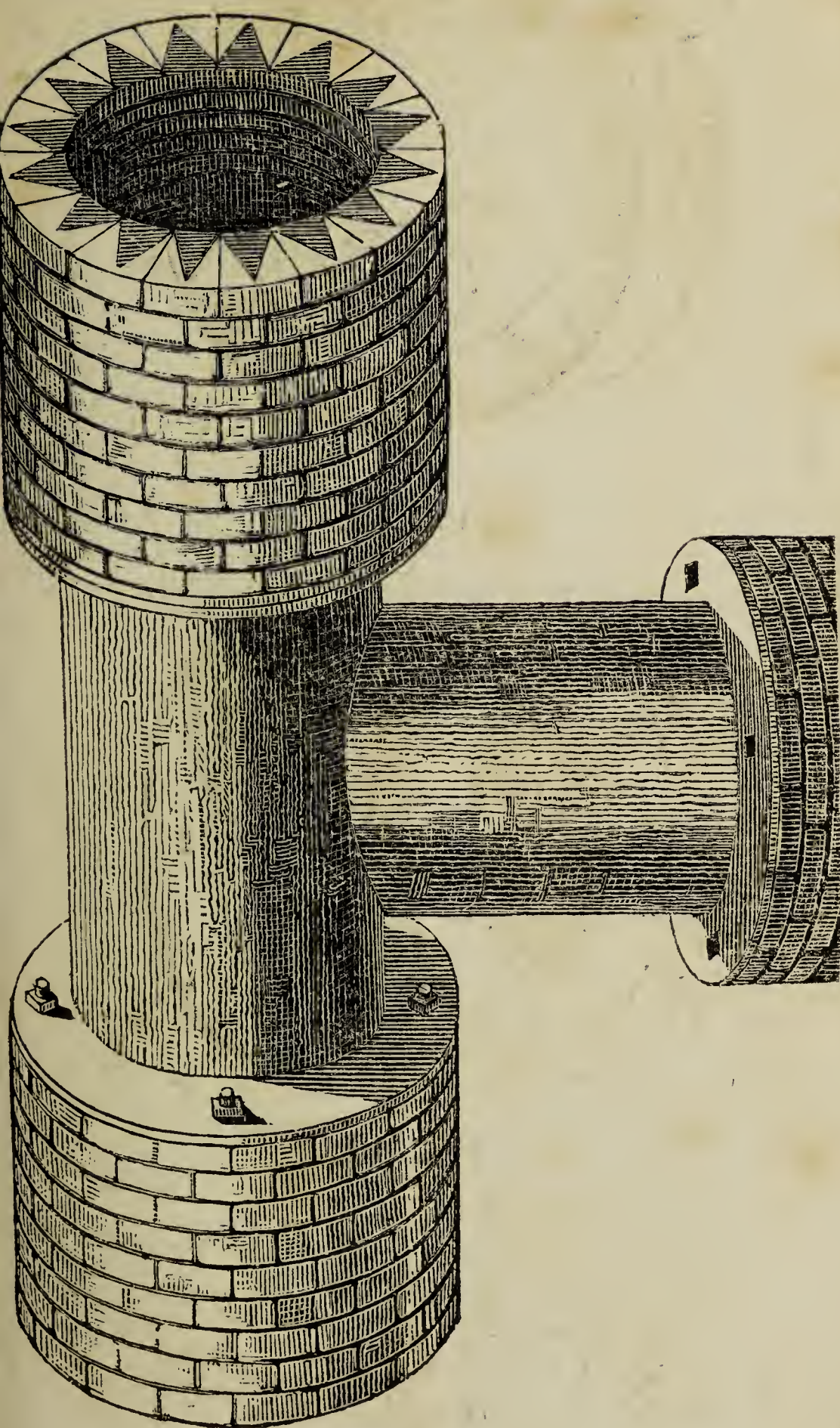


FIG. 11.

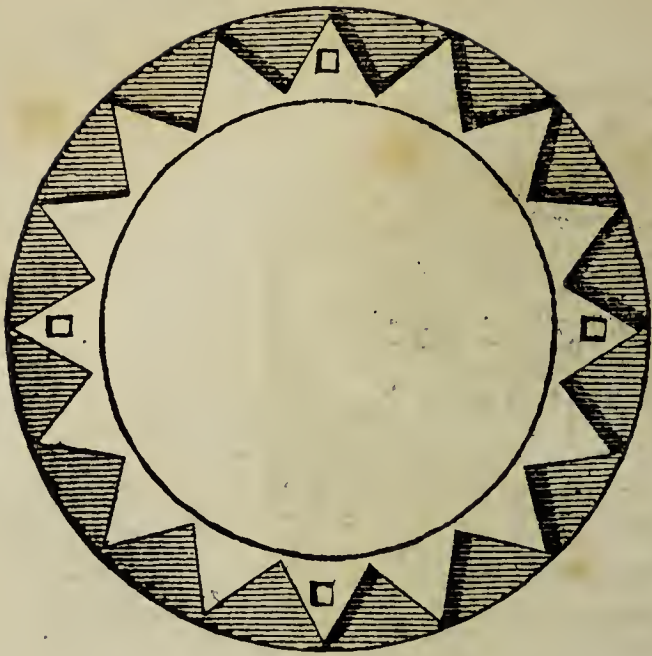


FIG. 12.

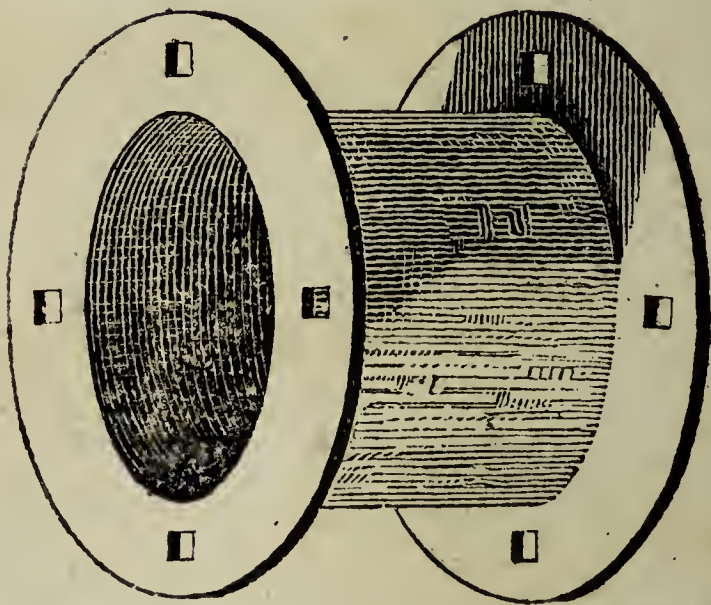


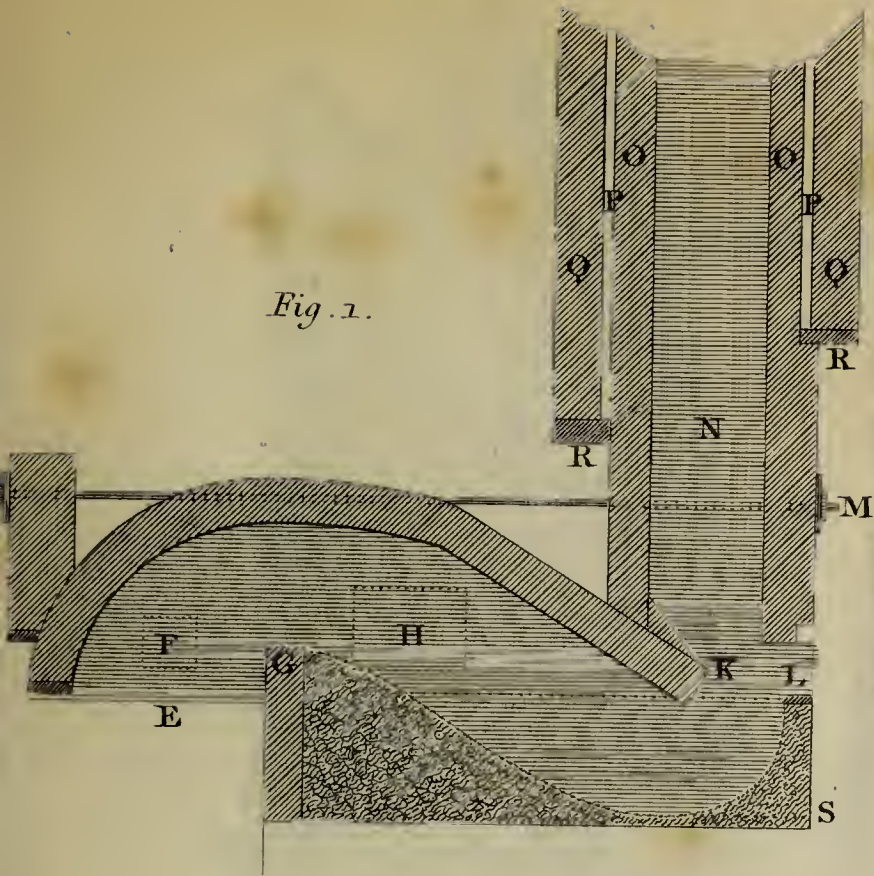
FIG. 13.



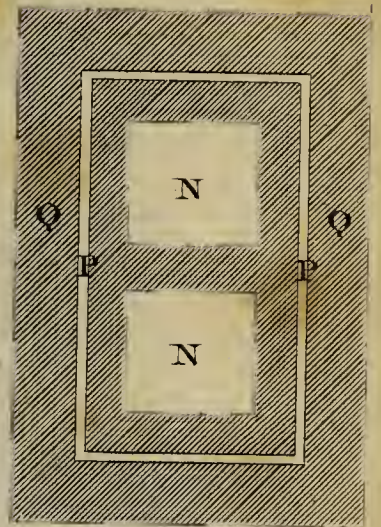




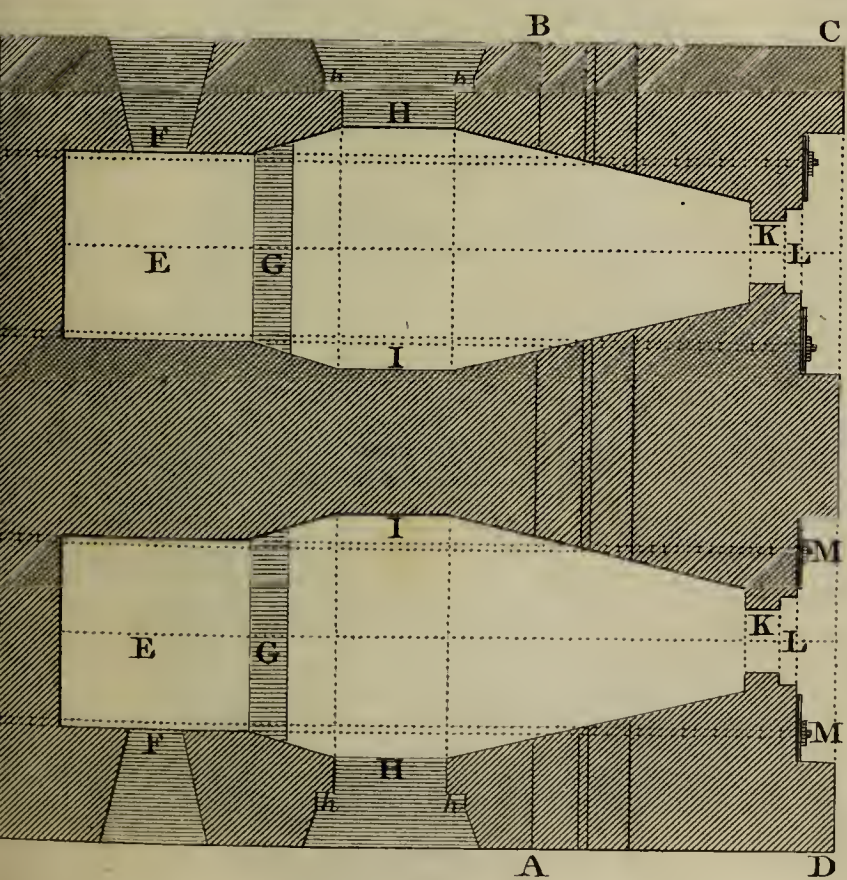
*Fig. 1.*



*Fig. 3.*

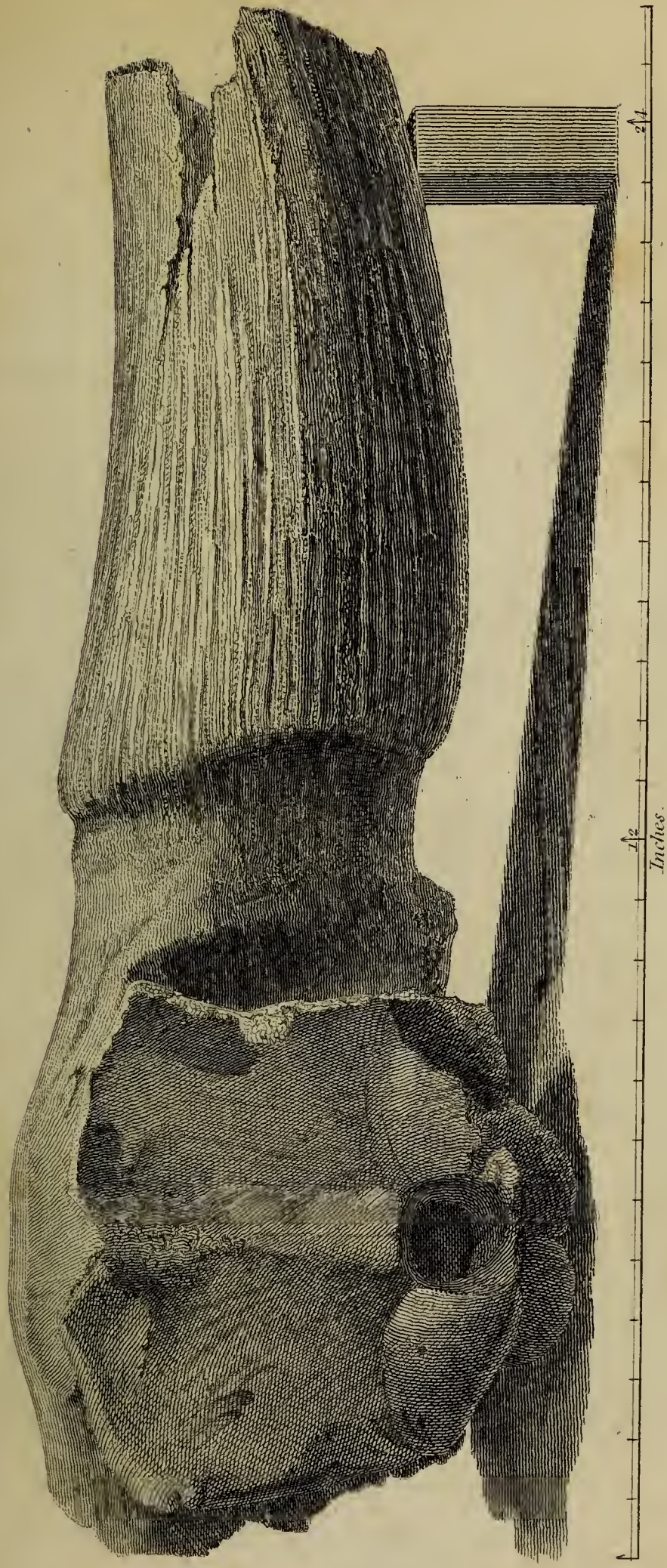


*Fig. 2.*





GREAT INDIAN BUFFALO.



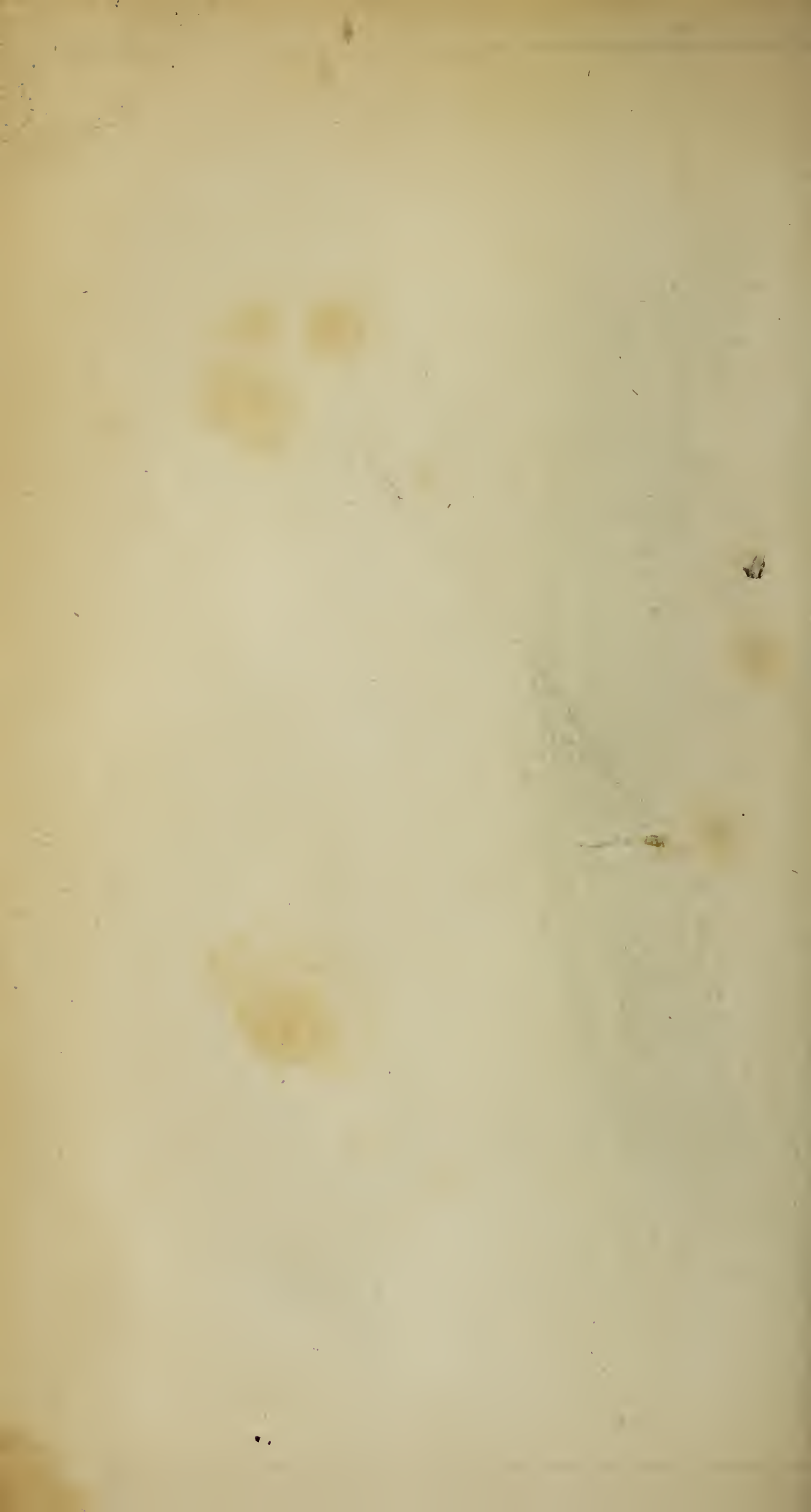




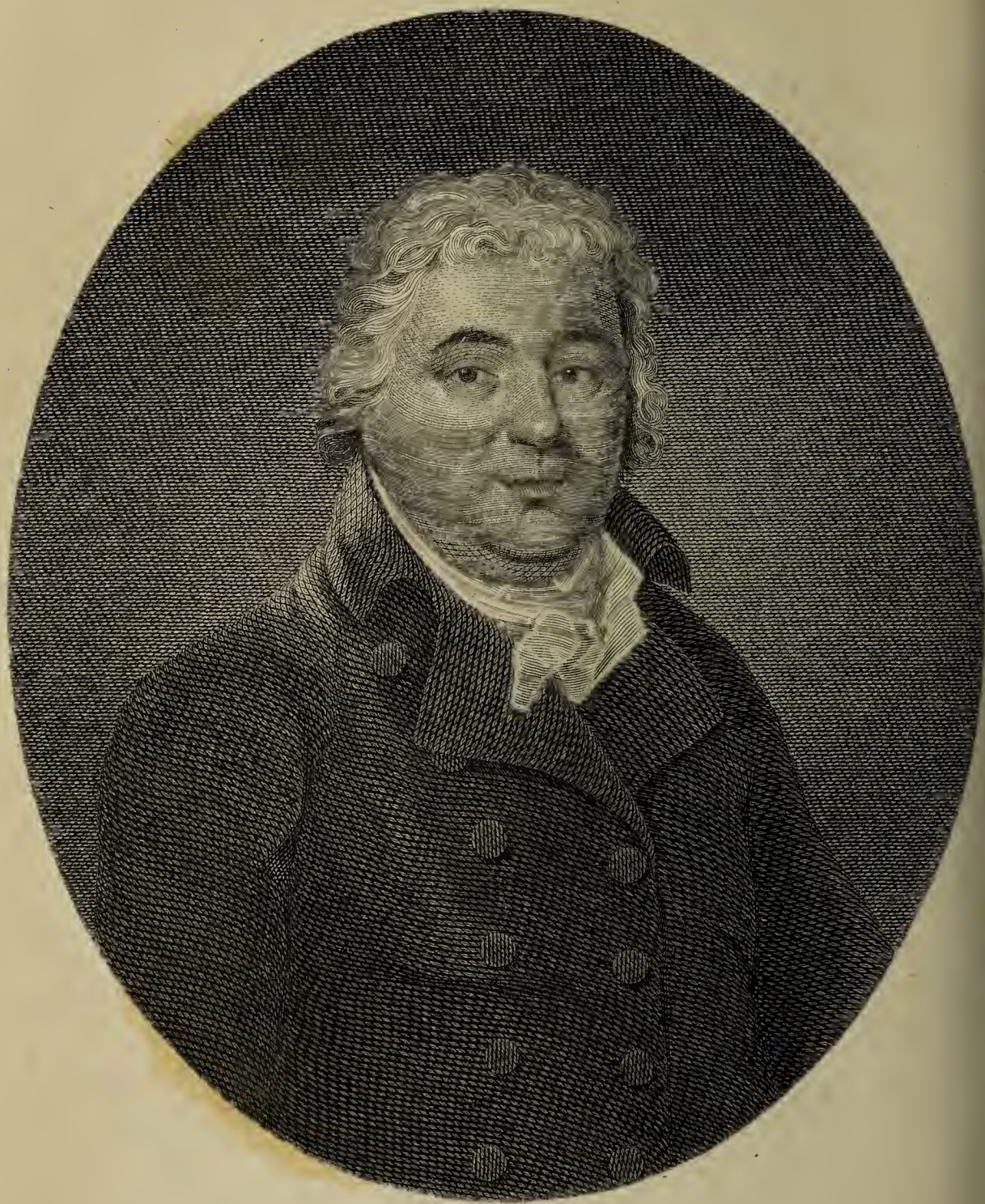
*NYMPHÆA caerulea.*











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